cold fusion txtfile

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Dear Colleagues, 1 November - 6 December1992.

COLD FUSION UPDATE No. 7.

THE THIRD INTERNATIONAL COLD FUSION CONFERENCE.

Held in Nagoya, 21 to 25 October 1992.

It started with a NTT Press Conference but ended with a Whimper.
Cold Fusion is now claimed with NORMAL hydrogen.
Fewer published results but more funding.
Skeptics of Cold Fusion verbally attacked.
Original experiments of Fleischmmann and Pons and Jones criticised.
Fourth Annual Conference scheduled for Hawaii - scientific meeting?
OTHER NEWS - Lawyer Triggs writes to Frank Close.

SUMMARY

The character of the annual Cold Fusion conference is changing. In the Invited talks, only a few new results were presented which claimed excess heat and nuclear products while many other claims were relegated to poster sessions. These other claims included several groups saying that they observed excess heat with normal water, ie light hydrogen - this is in contradiction with Fleischmann and Pons and others who said it happens only with heavy hydrogen (deuterium) and the proof that it is nuclear fusion is that it is NOT observed with light hydrogen.

There were a number of highly unusual papers available but not all presented, claiming Cold Fusion in biology, in tiny black holes, in gravity decays and a Purdue group claimed it would help the Solar Neutrino Problem; also transmutation was claimed. On the other hand the most complete experiment in Japan according to the book of Abstracts, has been carried out over three years by Isagawa et al. at the National Laboratory for High Energy Physics, KEK - it was not chosen for presentation and was not mentioned - their evidence on excess heat, neutrons and tritium was against Cold Fusion although they found many artifacts which at first had appeared as real effects.

Near the start of the conference, Nippon Telephone and Telegraph, NTT, held a press conference where Drs. E. Yamaguchi and T. Nishioka announced that they had for the the first time succeeded in detecting excess heat and helium during the experiment with high reproducibility. According to the three-page article in Liberation of 27 October, this caused the NTT share price to rise by over 11% (note NTT has the biggest share capitalisation in Japan and at times in the World - the rise was worth some eight billion dollars). The NTT share price fell quickly on subsequent days. Also it was said that the helium was observed with deuterium and not with light hydrogen. However at the Conference Round Table, Dr. Yamaguchi said that they had also observed excess heat with light hydrogen. With other contradictions, it is probably wiser to consider the press conference as premature since adequate checks have not yet been made and the evidence for excess heat is uncertain

(see below). However NTT have offered to sell a Yamaguchi-style kit for $565 000 and they forsee a Nobel prize for Dr. Yamaguchi(see Notes).

S. Jones and H. Menlove have tried to detect neutrons in the large (3000 ton) Kamiokande detector; with palladium and titanium, upper limits corresponding to 10 E -14 Watts were found. Previously they had claimed to have observed two types of bursts, some lasting for a few hours and the others lasting only about a hundred microseconds. However such effects were not observed in Kamiokande and with the much lower background, all the previous claims were disproved. They then tried cement in Kamiokande and not unnaturally in view of the high radioactivity of cement, observed counts.

The fact that Cold Fusion is observed in some parts of the World but not in others - called in an earlier Email "the Regionalization of Results" - continues with Dr. F. Scarramuzzi saying that "Behind the Alps, Cold Fusion never existed".

An expert on hydrogen in metals, Dr. F. Fukai, explained that Cold Fusion at the rates quoted, was impossible. D.R.O. Morrison reviewed all the published papers (over 700) and noted that the numbers of papers published had declined steeply and that only 8 experimental ones have been published so far in 1992 and of these 6 found no effect, one was positive and one undecided; he concluded "It has been said that if Cold Fusion has a 1% chance of working, we should continue. But the best estimate is not 1%. If one accepts the Kamiokande limit of 10 E-4 neutrons/second which is 10 E-16 Watts, then it is not 1% but (10 E-14)% or one hundred million millionth of a percent". Both Fukai and Morrison were verbally attacked by Cold Fusion Believers.

The meeting finished with a round table discussion where the speakers mainly said that better experiments should be done, and then people drifted off without any great show of enthusiasm.

One fact that was not clearly stated at the conference, is that the vast majority of the World's scientists do not believe that Cold Fusion could give useful energy and most do not believe in Cold Fusion. However this fact does seem to have been recognised as when one delegate said "Who has not been ridiculed by his colleagues?", there was a sympathetic agreement and no one objected.

A major question is:
"Can the Annual Cold Fusion Conferences be considered as scientific meetings"?

Mr. Triggs, the lawyer of Stan Pons, has written a curious letter to Frank Close about the facts of the curious way that the F&P peak at 2.5 MeV moved to 2.2 MeV and the scale moved from 100 to 200 keV bins

SUBJECTS

1. Before and Organisation
2. Thursday 22 October; McKubre, Claytor, Kunimatso, Srinivasan, Oyama, Enyo, Thompson, Fukai, Sanchez, Chien.
3. Friday 23 October; Takahashi, Mallove, Celani, De Ninno, Pons, Smedley
4. Saturday 24 October; Jones, Yamaguchi, Miles, Iida, Kasagi, Cecil, Tsarev, Gozzi, Morrison
5. After Morrison's Invited talk
6. Sunday 25 October; Claytor, Bockris, Li (China), Tsarev(Russia), Scaramuzzi(Italy)
7. Round Table, End of Conference
8. Next Cold Fusion conference - scientific meeting?
9. Conclusions.

NOTES

OTHER NEWS

1. BEFORE AND ORGANISATION

The conference was supported by 8 major Japanese Societies - one was the Japanese Physical Society which I know well and which I respect, so expected a normal scientific meeting with a balance of speakers chosen to present different points of view and expected free and open discussion. Though not

emphasised, there was clearly some appreciable Japanese industrial support for Cold Fusion.

There were some 320 participants which was substantially more than the first two meetings which had about 200 each. Of these 199 came from Japan and about a third were from industrial organisations such as Mitsubushi, Toyota, Fuji Electrical, Sumitomo Electric, Tokyo Gas, Hitachi, Tokyo Electrical Power Co., Osaka Gas, NTT, Honda, Nomura, Nippon Steel, Kansai Electrical Power Co., Sanyo Electric, Aisin Saiki Co., NKK Co., Central Research Institute of the Electric Power Industry, also the Japanese offices of two French companies (Air Liquide and Cogema), plus the Director and Deputy Director of the Electrical Power Division of MITI - note that most were observers and not reporting results. This was quite an achievement for Dr. Hideo Ikegami, the Chairman of the Conference. There were 55 listed from the USA, 20 from Italy, 16 from Russia and the Ukraine, 11 from China and only 19 from the rest of the World, (which includes Stan Pons listed as from IMRA in France and Martin Fleischmann listed from the University of Southampton and the only person from the UK) so that it can be seen that World coverage was non-uniform.

There were only 23 talks - all of 20 minutes except Stan Pons who had 30 minutes. There were reviews of Cold Fusion in China, Russia and Italy. Also there were two panel discussions and the meeting ended with a round table discussion. From the abstracts it seemed that I was the only skeptic speaking. 75 papers were scheduled for the poster session - again mine seemed the only paper presenting a skeptical viewpoint even though most of the World's scientists think Cold Fusion is dead. The poster sessions in the afternoons were of an unusual format - it was a very large room with many tables and the "posters" were generally A4 pages which covered the table. Thus the morning speakers could cover the table with their transparencies. This system worked very well and allowed everyone a satisfactory chance of seeing the papers and of discussing with the authors and with other participants. An afternoon was devoted to visiting the Toyota car plant - this was very interesting as while there was some robotization, what we saw was the production chain with many men doing various operations - they worked steadily but did not seem to be forced to go at too high a speed. At the end of the production line, the cars were driven a few metres and tested immediately. (We have had Toyota cars in the family for 19 years and they never break down so are thinking of buying another one next year - what I saw of the production chain reinforced this opinion).

At the spectacular conference dinner, the representative from MITI said that they would fund research in Cold Fusion in the near future (was told they would give about $2.5 million next year and industry would give a comparable amount) This was not to be taken that they believed in Cold fusion (they call it Hydrogen Energy research) but that they thought it was worth further study. A message was read from Minoru Toyoda who is a major figure in the Toyota car company. He founded Technova in 1978 and IMRA in 1985. After Technova received a joint research proposal from Professors Fleischmann and Pons, he judged that they should work for IMRA Europe at the Science park near Nice. IMRA Japan is now also working on Cold Fusion research. Mr Toyoda is like many of us, greatly concerned by the World Energy problems and desires a harmonious development of Science and Technology as proposed by President Mitterand at the 1982 summit. His message is a very sincere one. (A review paper "World Energy in the Next Century" which is based on my Invited Talk at the November 1991 World Clean Energy Conference and presented at the 1992 Pugwash meeting, was submitted to this conference but not listed nor displayed).

2. THURSDAY 22 OCTOBER.
McKUBRE, CLAYTOR, KUNIMATSU, SRINIVASAN, OYAMA, ENYO, THOMPSON, FUKAI, SANCHEZ, CHIEN.

Missed the Welcome Party on Wednesday 21 October as was at the excellent Neutrino Astrophysics conference at Takayama and Kamioka. We also visited the famous KAMIOKANDE experiment which detected neutrinos from Supernova 1987A and is measuring neutrinos from the Sun now. It has 3000 tons of

highly purified water and almost a thousand large photomultipliers in the walls which measure Cherenkov radiation. Steve Jones and Howard Menlove have been trying to repeat their Cold Fusion experiments in Kamiokande for over a year. Kamiokande is one of the best detectors in the World with a strong well-funded team led by Yoji Totsuka.

On arriving was told by several people that excess heat was now being observed in light hydrogen by several (five) groups - truely startling news as previously the evidence that Cold Fusion was fusion of deuterium, was that the excess heat effect was observed with deuterium but NOT with hydrogen! These new claims change everything. In addition was taken aside and told that transmutations were being observed! The alchemists dream come true. By an unfortunate coincidence, none of the people telling me of these results had been invited to speak though they could present their sensational results at the poster sessions. Also they were not in contact with the press as the press conferences were only for Invited Speakers.

2.1 The first talk was Mike McKUBRE of SRI who, as usual, gave an excellent talk. He said over 200 experiments had been done on the loading of deuterium into palladium. He showed a graph of the loading, (D/Pd = x) with a peak near 0.8 to 0.85 and a very broad shoulder going down to zero and a steeply falling slope down to almost 1.1. He claimed that just below x = 0.93, four experiments gave excess heat and two did not while above 0.93 all gave excess heat. He uses closed cells. One of the fair aspects of his talk is that he presented his excess heat results in terms of the three different ways of expressing them;

Peak excess heat observed for a short period of time = 350%
Average excess heat during bursts = 2 to 50%
Overall excess heat from start of run = 1 to 2 to 3 to 4%.

In proposing a system which would be useful for power production, it is the last figure of 1 to 4% which is the relevent one for power companies. It would be good if all groups would follow Mike's example and give their claims in three ways - maximum effect, average during bursts, and average since start of run. This comment applies to excess heat and to nuclear products.

2.2 Tom CLAYTOR of Los Alamos gave a serious talk on the work of Ed Storms who could not attend. He said they have claimed an excess heat of about 20%, but when asked privately, did not know if Storms had calculated the overall total excess heat from the start of the run. The highest loading Storms had been able to achieve was x = 0.82 but when he took out the used palladium rods and scraped off the crud, the maximum loading was only 0.68 - troubling if one wishes to use the palladium repeatedly over long periods in a power plant.

2.3 Next was Dr. KUNIMATSU of IMRA Japan Co. Ltd - this appears to be a research foundation set up by Mr. Toyoda who is closely associated with the Toyota car company. They have spent rather generously for the last two years on Cold Fusion research. They find a maximum loading of x = 0.88. Excess heat of up to 35% was observed.

2.4 Dr. M. SRINIVASAN of BARC, Bombay, gave a remarkable talk. He said that the hottest topic was the Mills and Kneizys result that excess heat was observed with H2O (light hydrogen) when K2CO3 salts were used with a nickel cathode and platinium anode. This, he said, was explained by Mills with a "crazy theory with compact hydrogen atoms." He said he did not believe it but tried and found that 17 out of 18 cells gave excess heat and many gave tritium. Also with other alkali salts, Li2CO3 and Na2CO3, excess heat was obtained. Three groups at BARC obtained these results. (Note - the abstract does not contain all these results and says checks are being done).

2.5 Dr. N. OYAMA reported on experiments on excess heat in closed cells (they abandoned open cells because "the evaluation of excess heat is complicated"). They observed excess heat of 2.42% or 0.57W/cm3. This was only in one cell out of five (does this mean the total excess heat was 2.42/5 = 0.484% ?). No excess heat was observed with hydrogen.

2.6 Dr. M. ENYO gave a technical talk where he tried to compare loading obtained by electrolysis with that obtained by gas pressure - his highest value was H/Pd = 0.9 (note this is H and not D - usually D/Pd is 5 to 10% lower

than H/Pd) by electrolysis which corresponded to 10 000 atmospheres. He said "the equivalent hydrogen pressure should not generally be related to the hydrogen overpotential by a simple Nernst-type equation" - this is in contradiction to Fleischmann and Pons who claimed an enormous equivalent pressure of 10 E26 atmospheres.

2.7 Dr. D.T. THOMPSON of Johnson Mathey gave a technical talk whose relevance was unclear - what people really wanted to know was how much helium was there in the Fleischmann and Pons Palladium rods after their claims in 1989?

2.8 Dr. Y. FUKAI talk was entitled "The ABC's of the hydrogen-metal system" and the abstract was rather calm. However the talk was shattering to Cold Fusion Believers. He noted that in D2 gas the separation of the atoms is 0.74 A and one needs a distance of 0.15 A to obtain 10 E-20 fusions per second. He explained why normally no two atoms can be closer than 2.1 A in a Palladium lattice. He also explained why it is difficult to load beyond 0.83. With vacancies in the lattice, it is possible to have up to 6 deuterium nuclei together but as Besenbacher showed, the separation is always greater than 1.85 A as some of the palladium nuclei are displaced by 0.3 A. Further he pointed out that undulations in the potential can reach about 1 eV but this is small compared with the 52 eV potential so that again the fusion rate will not be enhanced usefully. He showed that the use of a screened Coulomb potential was erroneous. It was suggested that before starting any new Cold Fusion experiment, one should read Dr. Fukai's book due out next January. Prof. Preparata of Milan said forcibly that something is missing - could you tell me why metals exist? You could not answer; and if you would answer I would shoot it down. People find heat. You think we are idiots but people find things.

I tried to find Dr. Fukai later in the meeting but was unsuccessful.

2.9 Carlos SANCHEZ from Madrid talked of deuterium concentration in titanium. He concluded that there was a limit to the loading of titanium at room temperature of 1.65 to 1.70.

2.10 Dr. CHIEN of ROC(Taiwan) presented remarkable results claiming the power out was 8 times the input power with values up to 100 times. He is now at Texas A&M where he now again finds remarkably large amounts of tritium.

3. FRIDAY 24 OCTOBER.

TAKAHASHI, CELANI, MALLOVE, DE NINNO, OKAMOTO, PONS, SMEDLEY.

THEORY PANEL.

3.1 PANEL of Drs Takahashi, Okamoto, Mallove, Celani and de Ninno. The main subject was the results of TAKAHASHI et al. who have claimed 200 W/cm3 excess heat using a new technique of rapidly varying the input power with a 12-hour period. However they are now unable to repeat their earlier high values. A very unusual result was that the weak neutron emission (one neutron/sec) was decreased when the excess heat level increased - contrary to all previous results. However Dr. Takahashi can explain it in terms of his model where fusion can occur between two, three and four deuterons - the potential barrier apparently being not important - many physicists find this a very remarkable theory. He also found particles of 3 to 5 MeV which is higher than usual theories give.

3.2 Dr. F. CELANI reported on attempts to repeat the Takahashi experiment - the results which are still preliminary, gave excess heat but of appreciably lower levels, about 10%.

3.3 Dr. Eugene MALLOVE reported on his and other experiments using the dynamic Takahashi technique(he is a journalist specialising in science who wrote the pro-cold fusion book "Fire from Ice"). He and Mr. Rothwell built their equipment for less than $10 000, found ambiguous effects during the first 60 days. In their second run with new palladium, they found heat balance to 5%, ie no excess heat. He said that Tom DROEGE has a wonderful thermoelectric device which is very accurate (DROM comment - Tom is a senior engineer at Fermilab and did a great job of work in the building of their CDF detector which is one of the most important Particle Physics experiments. His closed cell works as a null experiment like the Wheatstone Bridge, so that as excess

heat is observed, the input heating is decreased to preserve a constant temperature - this means that there are no complicated calculations of sudden artificial temperature rises induced to observe the subsequent cooling curve to calibrate the system. His system is accurate to a few mWatt. Between mid-September and mid-October, he observed excess heat of just under 1% which he described on the fusion electronic net, but since has found that there was a subtle drift in his calibration so that there was no excess heat. It is interesting that while most people who have announced an erroneous result and then found their mistake, do not announce this, Tom has make an equally public retraction on the net - a sign that he is a good scientist. Thus it seems difficult to confirm the first Takahashi result).

3.4 Dr. DE NINNO of Frascati described the transport of deuterium in Palladium. (She did not report any continuation of the Frascati experiments which caused such excitement in April 1989. These involved warming up from liquid nitrogen temperatures and claimed to observe neutron emission near -30 C).

3.5 Dr. M. OKAMOTO reported that they had repeated the Takahashi technique of low and high loading and found neutron emission from 4 out of 8 runs but of very low intensity. The neutrons had two components, a weak 2.45 MeV component and a stronger component at higher energy whose origin is uncertain. They believe that they have confirmed the Takahashi result (however the neutron intensity seems very different - paper awaited).

3.6 Stan PONS began his talk by showing a short video of four cells with different inputs. Each cell boiled off its liquid after a different number of days. The cells seemed to be operated in the 60 to 80 C temperature range - it was said that the condition for success was to operate near the boiling point. This worried some as the corrections are much larger at high temperature. Some felt this was impressive proof, others that there are many different ways to make a cell with palladium boil (eg G. Kreysa et al., J Electroanal. Chem. 266(1989)437). The demonstration was not convincing to scientists as it needed more information - one would like to see the demonstration repeated in the presence of someone like Tom Droege to watch and test and preferably also with several video cameras.

One striking feature of the video was the extremely small size of the cell, barely thicker than the thumb of the person holding it. Later the volume of the palladium was given as 0.0785 cm3 - this is much smaller than in the original 1989 paper where it was written that results for bigger cathodes of 2 cm diameter would be presented, but so far it seems that the palladium used is getting smaller and smaller rather than bigger. When a serious scientist who believes in Cold Fusion was asked about it, he replied that anyone who works in electrochemistry knows that it is better to have small electrodes. When it was suggested that this was bad for the commercial use of Cold Fusion in large, one Gigawatt, power plants, he replied "Ah". In reply to a question as to whether Cold Fusion was a surface or volume effect, Dr. Pons replied that it was a volume effect.

He said that they were just entering their new building at the IMRA technical centre and showed photographs of it. He said they had 32 employees.

What was remarkable about his talk is that he did not mention the recent paper by himself [redacted]S. Pons and M. Fleischmann, Il Nuovo Cimento, 105(1992)763 [redacted] entitled "Concerning the Detection of Neutrons and Gamma-rays from Cells containing Palladium Cathodes Polarized in Heavy Water". This is an interesting paper as it appears to be an attempt to answer criticisms and to discredit the experiment of Mike Salamon et al. which was done in their Utah laboratory beneath the table on which were 4 of their cells, and which found nothing. Since this was not presented, it will be discussed in the Notes at the end.

3.7 S.I. SMEDLEY of the Stanford Research Institute gave a talk on "Issues relating to the Safe Operation of Electrolysis Cells" which was mainly about the accident that cost Andy Riley his life. He said that many people had experienced explosive situations with the electrolysis of Palladium but until January without serious consequences. The pressure had risen to 30 atmospheres before suddenly rising to 300 atm. so that a six-inch steel cylinder hit Andy.

There was no radiation. He suggested various safety precautions such as having a strong shield against explosions and not removing the cell until it was sure that the cell pressure was one atmosphere. Also he was wearing safety glasses which surely saved his eyesight. He paid tribute to Andy as a fine person as well as an excellent materials scientist, which was also my opinion as I greatly appreciated Andy. This was not an easy talk to give, but Dr. Smedley gave a sympathetic and well-balanced report.

3.8 PANEL on Theoretical Models, HAGELSTEIN, PREPARATA, ROMODANOV, VIGIER

Each participant talked of his theory. The Chairman asked if there was a critical experiment. Peter Hagelstein replied that the observation of isotope shifts was critical, 6Li to 7Li, 10B to 11B, potassium to calcium. Guiliano Preparata said that Cold Fusion was complicated but the production of helium was crucial. Dr. Romodanov said the only way to prove Cold Fusion was to do reproducible experiments. Dr. Vigier said that this conference had proved that excess heat was obtained with heavy water and probably also with light water; the crucial experiment is to prove that light water gives excess heat - he predicted that it would.

Dr R.T. BUSH said that light water work is being done successfully and correlated with excess heat - Bush, Fusion Tech. 22(1992)301. It is called alkali-fusion since potassium is changed to calcium). Dr. MALLOVE said that light water works. Dr. Vigier agreed and said the interpretation was non-nuclear and one should vary the mixture of D2O and H2O to prove it. Dr. Hagelstein said that claims of 200 MJ/mole implied 200 eV per atom which was difficult to explain by chemical means.

Dr. Preparata was asked if his theory was different from ordinary quantum theory and if so what what other predictions were made. He replied many. Dr. Chubb cried "That's wrong, that's wrong". Dr. Preparata, equally strongly, cried "It is right".

4. SATURDAY 24 OCTOBER

JONES, YAMAGUCHI, MILES, IIDA, KASAGI, CECIL, TSAREV, GOZZI, MORRISON.

4.1 Steve JONES started by emphasising that there was no correspondance between the claims of excess heat and nuclear products - if the excess heat claims were true, then for one Watt, about 10 E12 reactions per second were required which would yield very large amounts of nuclear products or ash. Thus for the DD reaction to give helium, some 2ml of helium should be produced if this reaction gives 100MJ of energy, which should be easily identifiable.

The critical test was observation of X-rays. They had done experiments showing that the K(alpha) spectrum had a strong peak at 21 keV from palladium. The attenuation in D2O liquid was only 60% after 2 cm. For one watt, the 3 MeV protons should produce 600 K(alpha) X-rays per second. They had now made a small X-ray detector which could be fitted inside an experiment. He has offered this to Dr. Takahashi who agreed and a test will take place on Tuesday October 27th (Note added - Steve went to Dr. Takahashi's lab as agreed with his X-ray counter; but could not perform the measurement of X-rays - Dr. Takahashi could not make his experiment give Cold Fusion plus Steve had some trouble with his counter).

They are running neutron counters in a low background (0.4 counts/hour) tunnel (they call it Pico Gran Sasso) and have observed 12 neutron bursts. The Provo Canyon lab has been set up with the support of EPRI and is a $1 million facility. He said that they are given encouragement and advice from Al Mann, Steve Koonin, Charles Barnes and others (suspect this does not mean that these three people believe in Cold Fusion).

Steve Jones reported on their work in Japan where they are running in the Kamiokande water Cherenkov detector with cells with cement in them as they are interested in Cold Fusion in Mother Earth. They ran in March/April 1991 and from October 1991 to February 1992. When loaded with deuterium gas they observed neutron bursts, but not with hydrogen gas. With electrolysis and titanium loaded with D2 gas, the number and multiplicity of the bursts is different from that expected from uranium contamination. A plot was

shown of the multiplicity observed and that predicted from the earlier Menlove et al. results where some agreement was obtained for low multiplicities up to 20, but not for high multiplicities. These results are being checked in the BYU tunnel where they are using fast-setting cement which allows the cells to be prepared in one day and not weeks. The statistics are low but agree. He finds this a good trigger for Cold Fusion and intends to continue these studies in Utah.

Note; The initial reason for the Kamiokande experiment was to repeat the Jones et al. and Menlove et al. experiments using palladium and titanium. These experiments which occupied most of the running in Kamiokande, were not mentioned by Steve but are discussed in section 4.9 below. In earlier versions it was intended not to discuss the cement experiments for reasons of politeness, but following remonstrances and claims that the cement results are very important, it seems that some comments are required though reluctantly.

The aim of the Kamiokande experiment was to measure nuclear products from Cold Fusion cells, these nuclear products producing Cherenkov light which is ultimately detected. Many nuclear products can do this - gammas, electrons, alpha particles, neutrons, protons if energetic enough. The Jones experiment was designed with the hope that the events would be mainly produced by neutrons, though for example, 24 MeV gammas would also give events.

Kamiokandee has made an enormous effort to remove radio-activity from the water, from the air (since radon is always present in mines), and from the materials used. To put palladium and titanium which are low in radio-active contamination is reasonable and a fairly clean experiment can be done and interpreted in an unambigous way. However the same cannot be said about cement. Everyone working with neutron detection knows that cement and many other common construction materials contain large amounts of radio-active materials and should be avoided. The random neutron emission was so high that the Kamiokande group found that it was interfering in a significant way, with their solar neutrino studies and asked them to desist (Kamiokande measures about one neutrino cominf from the Sun per ten days roughly, which is why they make such an effort to reduce radio-active background).

Recently on the Email net John Hawkinson gave a reference; "Radioactivity in Consumer products" NUREG/cp-0001, US Regulatory Commission, August 1978. Some numbers; generally cements have 1.1 pCi per gram for U-238 and 0.4 pCi per gram for Th-232. Gypsum from phosphate mining has appreciable radioactive ores and phosogypsum from the manufacture of phosphate fertilisers gives phosphogypsum and 20% of that goes into Portland Cement. Phosphogypsum from Florida has 33pCi/gm of Ra-226, 6pCi/gm of U-238, and 13 pCi/gm of Th-230; from other states other rates. Since these cements can produce a variety of radio-active decay or fission products, it would seem normal to avoid them since it is unlikely that one can do a clean experiment where one can interpret the results with confidence.

The argument has been made that "Mother Earth" has Cold Fusion and that is why cement should be used. Some points;

A) the justification for this hypothesis is that isotope ratios vary from place to place in the earth - but it is normal to expect variations in isotope ratios with atom bomb tests, accretions of radio-active material, cosmic ray reactions, etc. Such normal causes should be invoked before assuming Cold Fusion.

B) One should always try to arrange an experiment so that as far as possible one knows what one is doing so that the results can be interpreted and have some meaning. For example should one wish to study "Mother Earth", one should start by finding out what "Mother earth" is. Most people do not consider cement as "Mother Earth".

4.2 Dr. Eiichi YAMAGUCHI of NTT said many researchers had succeeded in finding evidence for Cold Fusion but no one had direct evidence for nuclear products detection "in situ". Now for the first time they have succeeded in the real time observation of helium using a quadrupole mass spectrometer

of high resolution (0.001 amu at 4 amu). The amount of helium gas was strongly correlated to the excess heat evolution and increased with increasing the loading ratio of D to Pd. Also tritium production has been observed as HT. But when the system is loaded with hydrogen, H, neither helium or tritium production is observed. Simultaneous measurement of charged particles gave alpha particles of 4.5 to 6 MeV as well as protons of 3 MeV but as the amount was extremely small relative to helium production, this "strongly suggests" the occurrence of a new class of nuclear fusion in the system Pd:D(H). These are remarkable claims, so was very surprised to hear at the Round Table next day Dr. Yamaguchi give some additional results when he stated that with hydrated palladium (ie with light hydrogen), a heat increase was observed but neither helium nor tritium was found(note that this is in their paper).

They interpret their results by suggesting that the main reaction is;

d + d ---> 4He + photons/phonons

This follows Nobel laureate Schwinger who said in Z. Phys. D, 15(1900)221 that the reaction

p + d ---> 3He + photons/phonons

was favoured over the dd reaction. Since these reactions are less than the normal strong reactions giving neutrons, tritium etc., then a "new class of nuclear fusion is required in the system Pd:D(H)." This is a very strong statement in their abstract.

Their basic idea is to cover one surface of a thin palladium plate with an oxide barrier which is a surface barrier for the out-transport of deuterium, then the Pd plate is loaded with D2 gas of about 0.5 atmospheres giving a measured loading of about x = 0.6. Then a thick gold film is deposited on the other side to prevent d ions escaping there. Then a vacuum is created on the oxide side. They mention their earlier work, reported at the BYU conference (AIP 228(1990)354) where they claimed "gigantic neutron bursts" of a few million neutrons for a few seconds and excess heat, but the only evidence that they presented for excess heat was the statement that the gold annealed to the palladium which they estimated gave a temperature of 800 C. This is not usually considered evidence for excess heat as normally careful controls and checks are done. In the next tests, strong currents - 5 to 7 A are mentioned - were passed through the plates. There are two plates, A and B, which are said to be "equivalent" but the curves of temperature variation are different and no comment is made. The maximum temperature measured now, is about 200 C and this seems to be taken as evidence of excess heat - again no controls and calibrations are reported and no estimates of the amount, eg watts/cm3, are given - unusual.

The mass spectroscopy is only done near mass 4 (Dr. Yamaguchi said it takes about a week to set up as it is so precise) and peaks are expected at 4.00260 amu for 4He, 4.02388 amu for HT and 4.02820 amu for D2 - the accuracy claimed is 0.001 amu. It is claimed that with D2 gas a peak is seen at the HT mass value and this is evidence for tritium - but this peak is bigger than the D2 peak which is remarkable as it means the Tritium has to be produced in very large quantities and has to be very efficient in finding H ions to give such a big peak at the HT mass. Peaks are observed developing at the 4He mass as time increases - a major question is whether there is any glass in the system for Nate Hoffman said he has spent 6 months repeating the Paneth and Peters 1926/27 experiment and has shown that glass always contaims some helium and if hydrogen(or deuterium) gas is passed over the glass then some helium comes out and will give a signal - a question that is not clear. Again no measurements are given of the quantity of helium and tritium produced. (Note - at first the presence of glass in the apparatus was denied, but it seems that there was probably some - Drs. Scarramuzzi and Sanchez will be able to say since they were to visit the NTT laboratories after the conference and their reports are eagerly awaited).

The charged particle detectors give rather poor statistics (one peak seems to have four events in it) and the interpretation is unclear.

The plate undergoes plastic deformation which indictes that violent processes are taking place - experience has shown that such violent processes

can cause artifacts eg false signals in a neutron counter, and it is wise to perform many careful checks and quantitative measurements before claims are made.

Overall the experiment is unconvincing.

4.3 Drs M.H. MILES and B.F. Bush reported on a search for anomalous effects. While earlier 2 palladium rods gave excess heat 7 times out of 8, a batch of 8 new palladium rods gave no significant excess heat. Studies of helium production are hindered by this - earlier measurements gave 2 E11 helium atoms/sec. Increases in tritium could be explainable by normal enrichment during electrolysis.

4.4 Dr. T. IIDA (and Dr. A. Takahashi et al.) reported work with deuteron plus He and H beams of 240 keV (ie lukewarm fusion). In addition to the expected particles they also found peaks at 3.6 and 8.0 MeV of alpha particles. Dr. TAKAHASHI explained these surprising results as being caused not by lukewarm fusion but by Cold Fusion with multibody reactions, eg ddd, pdd where the three ions react together according to the theory he has developed to explain his surprising results (it is not clear how the high multi-Coulomb barrier is overcome).

4.5 Dr. KASAGI studied lukewarm fusion using deuterium ions of 150 keV. Some unusual peaks were observed.

4.6 Dr. F.E.CECIL - sorry have no notes on his talk but the abstract says that charged particle emission was studied with silicon surface barrier detectors, from titanium/palladium cathode glow discharges in D2 gas. The voltage varied between 500 and 3000 V. It was concluded that some of the observed burst events appear to be real particles from nuclear reactions at the cathodes while others appear to be electrical pick-up by the detectors from the randomly occurring sparking.

4.7 Dr. V. TSAREV was replaced by Drs. KALIEV and KUCHEROV who gave talks claiming strong alpha particle and gamma emission which was reproducible. Nate Hoffman commented that the waste material from Russian reactors contains a high fraction of palladium - this waste material has been used to extract palladium which is sold commercially at a very reasonable price. Hence anyone using Russian palladium should check whether it was highly contaminated with radioactive decay products which would give off many alpha and other decay products.

4.8 Dr. GOZZI reported that they had 60 neutron detectors which in the 15 days 29 September to 12 October had given bursts of multiplicity up to 340 and which were in coincidence with excess heat. No tritium was observed (note this is the opposite of many groups who claim that tritium production is a thousand to a hundred million times stronger than neutron production). Steve Jones said that as neutrons were slowed down in the polythene to thermal velocities, they should have been observed in all neutron counters not in just one sector. Preparata - "You calibrated it". Gozzi replied by showing a graph of the efficiency as a function of the group which showed that the efficiency was very low except in a few groups where it rose to 0.06. Steve said he still did not understand and they should discuss it later.

4.9 D.R.O. MORRISON gave a review of Cold Fusion Experiments. He emphasized the Universality of Physics - the same physics laws apply on earth, in the Sun, in Supernova, in pulsars where the density was 10 E14 times that on earth. He recalled the basic reaction chains in the Sun noting that dd fusion was not important, though if its rate was increased by 10 E 40 as some suggested this might be noticeable. The dd reaction gave a compound nucleus which lasted about 10 E-20 seconds before decaying and it always decayed the same way, independent of its formation. The two main strong decays were to (3He + n) and to (t + p) with a 1 to 1 branching ratio while the electromagnetic decay to (4He + gamma) was lower by a factor of ten million (Frank Close explained to me that there is another factor from spin apart from alpha, the fine structure constant). This had been shown experimentally at the Second Annual Cold Fusion Conference by Davis et al. who confirmed the ratios of 1:1: 10 E-7 down to about 2 keV. Dr. Preparata intervened and loudly said the speaker was insulting us, this was an academic lecture and was all well known. After a

pause, the speaker continued and noted that these branching ratios of neutron to tritium of one to one and helium4 being ten million times less, had been confirmed at zero energy by muon catalysed fusion at which subject Steve Jones is a world expert.

With dd fusion, the primary products were neutrons, tritium, gammas, 4He, 3He and protons while an important secondary product was X-rays of 21 keV produced when energetic charged particles such as 3 MeV protons, passed through palladium. The first four of these (n, t, gammas, 4He) all had major problems due to the ease of artifacts producing false readings. However 3He, protons and 21 keV X-rays were relatively clean and reliable measurements.

As Cold Fusion is potentially so exciting, many fast experiments have been done and presented before all checks have been made. Corrections and retractions are not always presented using the same media. The problem is how to get a fair unbiassed set of data to review. Have used the bibliography of Dieter Britz which most people consider unbiassed. He takes only papers which have been published and which therefore have been refereed. The set is up to 3 October 1992. It contains 727 relevant papers of which 256 are experimental results, 239 are theory and 232 are Others(64 reviews, 76 technical, 35 comments, 6 rebuttals, 36 repeats and 15 not cold fusion, eg lukewarm fusion). The Experimental papers were 86 positive(ie supporting the existence of Cold Fusion) and 136 null papers (finding no evidence and giving upper limits) while 34 were indecisive or contradictory.

There was a problem that some papers were very poor (eg 2 standard deviation effects, no hydrogen control, no calibration, only one neutron counter, no check for artifacts, etc.) but to be as kind as possible to Cold Fusion, and to avoid any accusation of bias, all were taken as evidence of Cold Fusion if the authors said they were evidence.

A page of 11 figures was shown giving firstly the numbers of papers as a function of the year - for experimental papers there were 72 in 1989(9 months), 128 in 1990, 48 in 1991 and 8 in 1992(9 months). Of the 8 in 1992, 6 were null, one was positive and one was indecisive. Thus it can be seen that interest in Cold Fusion peaked two years ago and is fading fast.

Secondly on this page, the numbers of results for each kind of effect (excess heat and nuclear products) were given. For each effect the number of null results was greater than the number of positive results. For the case of the three products which were relatively free from artifacts, the numbers were;
Protons - 11 null and one positive
3He - 8 null and one positive
X-rays - 7 null and zero positive.

Although one says "do good experiments", many are still inadequate. To list these is unsocial, hence the other alternative was adopted and good experiments were selected. One criterion is number of effects measured - it was shown that when many factors (eg excess heat, neutrons, tritium etc.) are measured simultaneously, null results are much more frequently obtained. Again the 727 papers listed were studied to see which ones Dieter Britz had considered as "expert" - note this was his opinion, not that of the author. The names of the first author of 'expert' papers are; Aberdam, Armstrong, Bacej, Baranowski, Bennington, Besanbacher, Blaser, Bulloch, Case, Cheek, Chemla, Divisia, Flanagan, Gottesfeld, Hayden, Ilic, Kreysa, D. Lewis(not the Lewis from Caltech), McCracken, Menlove, Morrey, Naerger, Olofsson, Paneth, Porter, Riley(who died tragically), Rugari, and Williams. It is to be hoped that serious students of Cold Fusion have already read most of these papers, or if not, will do so soon. These papers are classifed as one positive, 19 null, 2 unclear and 6 technical.

Another criterion of good experimental technique, is that the authors make a point of saying that they looked for artifacts. Dieter Britz mentions 18 such papers which are composed of one positive, 14 null, 2 unclear and one technical. Again most careful workers do not find any Cold Fusion effects.

As loading is said by many to be crucial in achieving positive Cold Fusion effects, the 727 papers were scanned for values of loadings measured. 52 papers reported loadings - of these 16 were technical and 36 experimental; these

36 gave 3 positive, 31 null and 2 unclear. Taking only the graph of loading by electrolysis of palladium, there is a broad peak in D/Pd near 0.8 to 0.85. Many authors comment that there seems to be a maximum loading. This graph is very similar to that of Mike McKubre with a peak in the same place near 0.83, but with his higher statistics, his plot extends to higher values of just over one, and also has a much wider tail down to zero (being unpublished the McKubre results do not qualify). Other results quoted this week are Claytor D/Pd = 0.82, Kumimatsu 0.88, Enyo 0.9 and Fukai 0.83. A further point is that it seems a surprisingly high proportion of experiments with positive results do not measure their loading.

Note - the most reliable method of measuring loading is by diffraction - the best is neutron diffraction though X-ray diffraction can also be used. This could be used as a calibration for other techniques such as resistance measurments, but these all have problems and should be considered as having appreciable errors which vary with time and conditions. In one experiment the cathode extended outside the cell and diffraction measurements were made on this extension, but it was not too clear how one was sure that the loading inside was the same as that outside.

From a review by Ed Storms, a graph was shown of the log of the number of neutrons against the log of the number of tritium atoms - it could be seen that there was no correlation, the ratio of tritons to neutrons varying from one thousand to one thousand million. A different explanation is that if there are three standard deviation fluctuations in the measurement of neutrons and three standard deviation fluctuations in the measurement of the tritium, then such ratios are expected - the reason is that neutrons are measured directly whereas since tritium has a half-life of 12 years, only the very small fractionof tritium atoms which happen to decay, are measured during the short time of the measurement. That is, this tritium/neutron ratio is consistent with there being no Cold Fusion, only fluctuations.

Another graph is of the log of excess power, watts/cm2, against the current (linear scale); a line is drawn which does not fit the data but does indicate that as the current is increased there is a saturation in the Watts/cm2 at about one watt/cm2 which seem contrary to the idea that if only the current density is high enough, then the loading will pass some critical threshold and Cold Fusion will occur strongly. Another interesting point about the graph is that it shows the original values of Fleischmann and Pons who found considerable excess heat at the very low current density of 8 mA/cm2 (indeed in their paper they wrote over 1000% excess heat is obtainable but the only occasion was with the lowest current density of 8 mA/cm2). The point is that Dr. R.T. Bush finds that they obtain excess heat with normal light water but when Morrison asked him whether this was in contradiction to Fleischmann and Pons who find excess heat only with deuterium and believe that it is fusion because they do not find it with light hydrogen, Dr. Bush replied that it was different because he works only at very low current densities, 1 to 20 mA/cm2, he said. However it was pointed out that Fleischmann and Pons also obtained excess heat in that region with 8 mA/cm2. Dr. Bush then pointed out that he used nickel and not palladium, but Morrison asked if in his theory, were nickel and palladium not the same - Dr. Bush replied that they were and therefore light water should have given excess heat with palladium (please note that the statement of the equivalence of nickel and palladium in this context, was a theoretical statement of Dr. Bush and not by anyone else).

It is surprising at this conference that people do not jump up to point out the contradiction that some people use light hydrogen as a control and find no excess heat while others do find excess heat with light hydrogen.

In March 1989 in Utah, the press conference announced that Cold Fusion gave both excess heat and fusion products, that is it was a fusion process in which mass was converted into energy. There were great hopes of a "Clean, virtually inexhaustable source of energy" - though it must be said that Martin Fleischmann demurred and was more cautious. However it was quickly realised that there was an enormous contradiction as one watt of power should have given a million million nuclear reaction products per second which would have killed

everyone around, but the measured nuclear products were many orders of magnitude less - about a million million times less as Steve Jones pointed out. Thus Cold Fusion claims split into two parts;
a) Excess heat - Fleischamnn and Pons - Watts/cm3
b) Fusion Products - 40 000 neutrons/second according to F&P
- 0.4 neutrons/second according to Jones
but 10 E12 neutrons/second were expected if fusion.

An important point is that both Martin Fleischmann and Steve Jones said that there was no secret - just a simple table-top experiment as one said. Thus to obtain Cold Fusion there was no need for any dynamic process such as heating, cooling, varying current as in Takahashi style. It should work just by simple electrolysis even at low current densities such as 8 mA/cm2.

Now these two original experiments have been severely contested over the years and it is clear that if the two original experiments which began the current Cold Fusion excitement, are shown to be untenable, then the very foundations of Cold Fusion should crumble. In addition to these earlier criticisms, recently two major results have appeared that would appear to contradict the two foundation results. It is important to consider them and their rebuttals.

Initially Steve Jones et al. reported in Nature in 1989 that in 14 runs, one of the runs gave a neutron rate of 0.4 n/second for 7 hours; this value was re-evaluated later to 0.06n/s by taking the average over all 14 runs (from this one can calculate that the total running time of the 14 runs was about 47 hours but have been told recently the value is 79.3 hours) This was using electrolysis with a palladium cathode. Later Steve and Howard Menlove did another experiment with titanium which was lightly loaded with D2 gas, in which they claimed large neutron bursts of up to 80 neutrons counted (corresponding to 280 source neutrons after correcting for efficiency) in a time interval of 128 microseconds; they were especially frequent after cooling with liquid nitrogen and then in warming up, the bursts being observed near -30 C. They also observed two bursts of 17 and 5 hours in 1703 hours running, or one burst per 850 hours.
Thus there were three effects claimed;
FEW-HOUR BURSTS; three bursts have been observed of several hours duration - it may be noted that the latter two bursts are only about 10% higher than the background but are statistically significant, though it is not clear whether they could be the tail of a large statistical distribution.
MICROBURSTS; bursts of neutrons lasting less than 128 microseconds.
TEMPERATURE EFFECT; the microbursts are preferentially emitted near -30 C.
Note - it does not seemed to have been commented that these two types of burst differ in time by a factor of more than ten million - a theoretical explanation does not seem to have been attempted.

Steve Jones, Howard Menlove et al. have placed Cold Fusion cells in the centre of the 3000 ton Kamiokande detector. As the Kamiokande detector is in a mine (visited it on 21 October when at the Neutrino Astrophysics conference held at Takayma and Kamioka - the experiment is impressive) and as it has large veto counters and careful control of radon and other possible radioactive backgrounds, very low backgrounds are obtained, hence the previous values of 0.4 or 0.06 neutrons/second should now have been very clear. The experiment is described in a thesis by Taku Ishida which is admirably written and which explains all the corrections and results in great detail - it is well worth reading just for the pleasure of its clarity, apart from its interesting results.

They started running in January 1991. At first they tried electrolysis with palladium and titanium cathodes but observed almost nothing, then with titanium loaded with D2 gas and again observed almost nothing. They then switched to cement which gave so much activity that it was suggested that they continue elsewhere. The results are;
FEW-HOUR BURSTS; Ishida writes "Random neutron emission (ie few-hour bursts) beyond the background level has not been observed both for the cylinders (ie gas) and from the electrolysis samples." the numbers are;

Pressurized D2 gas
Flux upper limit = 0.00008 neutrons/second at 90% confidence
Total live time = 1310.7 hours
Electrolytic cells
April set, Flux upper limit = 0.000098 n/s at 90% confidence
Total live time = 387.2 hours.
April set, Flux upper limit = 0.000057 n/s at 90% confidence
Total live time = 569.7 hours.

Comparing these results with a total running time of 2267.4 hours, to the 0.06 neutrons/second claimed by Jones et al. in 47 hours, there would seem to be disagreement. (Further it may be noted that the mass of the titanium in the Jones et al. experiment was 3 grams whereas the average mass in the Kamiokande gas experiment was 339 grams).

MICROBURSTS

i) Menlove et al. made a claim to have observed bursts of neutrons in a time of 128 microseconds. In real numbers they claimed to have seen many with 30, 40, 50, 60 and even 80 neutrons in the burst. Correcting for efficiency they claimed between 10 and 280 source neutrons (below 10 was background). In the Kamiokande gas experiment there were zero bursts which gave 4 or more real neutrons, ie there were zero bursts giving 10 or more source neutrons.

This is the basis of the conclusion that the Kamiokande gas experiment is in disagreement with the Menlove et al. claims.

ii) In the Kamiokande electrolysis experiment, two bursts were found with a multiplicity of four. That is two bursts had about 11 source neutrons. But none were observed with between 15 and 280 source neutrons. That is no bursts were observed for most of the the region 10 to 280 and two were observed in a very small segment, 10 to 15. Now the Menlove et al. claim is for the range from 10 to 280 source neutrons - if it is correct, it should be correct for the entire range not just a little corner. This a major disagreement and is the basis for the conclusion that the Kamiokande electrolysis experiment is in disagreement with the Menlove et al. claims.

TEMPERATURE EFFECTS

With the experiments with titanium and D2 gas and warming up from liquid nitrogen temperatures, "bursts" of 2 or 3 neutrons were observed (with an extended interval of 500 microseconds, not 128 - not very important) but none of these occured during the warming up period. It is concluded that there is no evidence for a dynamic effect near -30 C as previously claimed.

There has been some discussion as to what the observed bursts of 2, 3 or 4 neutrons could be. This may be intersting, but in no way changes the three conclusions reached above.

One obvious interpretation was that this was radioactive contamination for uranium fission can give up to six neutrons and plutonium up to seven which gives about the observed multiplicity distribution, but not exactly. There have been claims that this may be a new phenomemon at an ultra low level. Maybe, but it should be noted that

a) Kamiokande does not measure neutrons - it measures Cherenkov light. When an atom fissions, it emits not only neutrons directly but the fission products plus the decay products of the short-lived elements formed. Thus there is also emission of gammas, electrons, alphas as well and, if these are energetic enough, they could also give Cherenkov light eventually. These simultaneous (<500 microseconds) emissions would change the rate and the multiplicity distribution. So the situation is complicated and not merely the metal of the cathode must be considered, but all the components including the brine and the deuterium.

b) It is not safe to use hydrogen in place of deuterium as a background because while gammas do not give photo-disintegration in hydrogen, they do give photo-disintegration in deuterium producing neutrons. Such photo-disintegration would give additional simultaneous neutrons which would change the rate and

multiplicity distribution.
(NOTE - as these comments were contested, a complete review of the Jones et al. experiments with palladium and titanium has been written and is issued separately - the conclusion is that all the experimental claims made in Jones et al. and in Menlove et al., are disproved by the superior Kamiokande experiment with its very low background).

The General Electric paper, Wilson et al., J. Electroanal. Chem 332(1992)1, includes Fritz Will as an author before he left to become Director of the National Cold Fusion Institute in Salt Lake City. It consists of two parts. Part 1 is experimental. It describes briefly a long series of experiments firstly repeating Fleischmann and Pons's experiments as exactly as possible (since there is no secret, this is OK), and then variations and improvements some of which gave very high quality experiments. They find no excess heat and no neutrons nor tritium nor 4He.

Part 2 is a very complete discussion of the analysis of the Fleischmann and Pons experimental data. They find that the excess heat is generally overestimated and that control samples using hydrogen which F&P claim gave no excess heat, should have indicated excess heat if the analysis had been performed as described. (More details of this are given in the Email "Cold Fusion Update No. 6).

The rebuttal of Fleischmann and Pons is given in the next paper, J. Electroanal. Chem 332(1992)33. it says that the paper of Wilson et al. is "a series of misconceptions and misrepresentations"... "gross errors". Then follows 20 pages of calculations etc. with the comments;

1) Fleischmann and Pons say that Wilson et al. "have not provided sufficient information". Agree, but one can ask GE for data and hope to get it. lt would be good for Science if both sides were to exchange data.
2) Wilson and others say that the use of non-linear regression analysis and Kalman filtering is unnecessarily complicated (F&P say it is standard but when the audience at Nagoya was asked if they had recently used a non-linear regression analysis to obtain excess heat - no one answered).
3) Fleischmann and Pons say that "the precise control of the level of the electrolyte is hardly feasible" and this justifies the complicated analysis, but if a closed cell is used, then the level is constant.
4) This argument between leading scientists is disagreeable - in view of the crucial importance of the Fleischmann and Pons experiment to Cold Fusion, it should be resolved. Fortunately this can be done simply by Fleischmann and Pons doing a clean simple experiment with few corrections in a closed cell immersed in 3 constant temperature baths as was done by their good friend David Williams at Harwell using the device used for evaluating the amount of plutonium in samples. This is a null measurement like the Wheatstone bridge, ie if excess heat is produced, the heaters that keep the 3 baths above room temperature, are lowered to keep the temperatures constant. Thus nothing changes in the temperatures so that no elaborate corrections are needed. Loading and nuclear products should also be measured at the same time.

It should be appreciated that the best way for Drs. Fleischmann and Pons to answer critics would be to obtain positive results with a clean good apparatus chosen to require few corrections as above.

Some Conclusions;

1. There is a major separation between experiments which measure excess heat and claim watts and experiments which measure nuclear products which find 10 E-6 to 10 E-16 watts.
2. The positive experimental claims are highly dispersed and inconsistent with one another. Some experiments are poorly designed and artifact-prone with the consequence that artifacts are claimed as results. Answer/recommendation is to do only good fully-instrumented and fully-calibrated experiments that need few and unimportant corrections. Always measure loading.
3. Several experiments claim that Cold Fusion occurs in normal light hydrogen. This is in direct contradiction with most previous Cold Fusion claims which said the reason one knew it was Cold Fusion was because it did occurred with

deuterium and did not occur with hydrogen. It is not possible to believe both sets of claims simultaneously.
4. There are an enormous number experiments which describe the behaviour of hydrogen and deuterium in metals and these show that the deuterium ions are further apart in metals than in D2 gas - as described earlier by Dr. Fukai.
5. The two original experiments of Fleischman and Pons and of Jones et al., are contradicted by the General Electric Company's paper of Fritz Will and others and by the Kamiokande experiment of Jones et al., respectively.
6. It has been said that if Cold Fusion has a 1% chance of working, then it is worth further study. But the best estimate is not 1%. If one accepts the results from the excellent Kamiokande experimental limit of 10 E-4 neutrons per second, then the limit is not 1% but 10 E-14% or one hundred million millionth of one percent.

5. AFTER MORRISON'S TALK

After Dr. Preparata's loud intervention, the rest of Morrison's talk was heard in silence, but after he finished the Co-Chairmen said nothing, but the Conference Chairman, Dr H. Ikegami moved swiftly across and removed deftly the microphone from the speaker's jacket and the battery from his pocket and then quietened the tumult and booing by declaring that he wished to apologize to the conference. He was surprised that a scientist of Dr. Morrison's international reputation could make such a ridiculous talk and so on.
A noisy crowd then surrounded Morrison so that it was difficult for the TV people to film this from close up. The loudest voices were essentially Cold Fusion propagandists and it is interesting that none of their questions or comments were direct to scientific issues but were of the nature "Have you looked at the raw data?" One particularly interesting question was "in your bibliography, did you include papers from 'Fusion Technology'?" This is interesting because this journal has a reputation of being rather kind to papers in favour of Cold Fusion - for example "Cold Fusion observed with ordinary water", "Observation of quad-neutrons and gravity decay during Cold Fusion", "Searching for tiny black holes during Cold Fusion" - was shown a photo of a black hole! The editor says that more papers are refused than accepted. The answer of Morrison was 'yes' - in order to be as kind as possible to Cold Fusion and to avoid accusations of bias, all journals that claim to have referees were taken, including Fusion Technology. (NOTE, have been told that the paper on Cold Fusion and Black Holes was rejected by a referee, but was still published to the referee's surprise - it will be interesting to hear further comments on this).

After a time Morrison was removed from the noisy crowd by an Organiser who said he should attend a press conference downstairs. There Dr. Ikegami was talking in Japanese to reporters. This went on for over an hour and the phrase "Morrison-san" was heard frequently. Afterwards the meeting broke up and none of the reporters asked Morrison any questions though they gave their cards.

After lunch there was the poster session. As requested, Morrison spread out copies of the 21 pages of transparencies on a table. Many gathered round and accepted copies of the page with 11 graphs summarizing the number of results. Dr Preparata came with his two acolytes and started attacking in a very loud voice - interestingly enough none of his comments were scientific and he did not question the accuracy of any of the 21 pages spread out.
One of his accolytes then started loudly and again none of his comments in any way questioned the pages on the table though he did say he was spokesman of an experiment. Dr. Preparata was offered a copy of the page of graphs - he took it and ceremoniously tore it across and then tore it again and again before moving away. Wonder if he also burns books?

After that the poster session proceeded peacefully with many friendly conversations and people were happy to have a copy of the page of graphs.
It was noticable that then and the next day, the serious scientists such as Steve Jones, discussed but that the principals and other propagandists avoided the poster table.

6 SUNDAY 25 OCTOBER

CLAYTOR, BOCKRIS, LI(CHINA), TSAREV(RUSSIA), SCARAMUZZI(ITALY)

6.1 Tom CLAYTOR showed a very interesting graph of the D/Pd ratios versus the gas pressure for many temperatures between -40C and +70C. In every case there was a tendancy towards saturation at near 0.8 loading though further additional pressure gave slowly increasing loadings. Also the loading was higher the lower the temperature. This is a basic graph that all are interested in. They used stacks of palladium and silicon and pulsed with a high current, and deuterium gas. Tritium was measured on-line and where it appeared, it was within 48 hours. The tritium production varied from 0.02 to 0.2 nCi per hour; it increased with current.

6.2 John BOCKRIS working with C. Chien and Z. Minevski, obtained remarkably large amounts of tritium as Chien had already found in Taiwan. Addition of fresh D2O or vibrating with a gold rod stopped the tritium production but after a few days it started again. Helium was also observed - about 1.6 E11 atoms. No 3He was observed.

6.3 Dr. X. LI gave an impressive list of institutions that are working on Cold Fusion in different regions of China. Several groups have positive results though the experiments are not too complex and there was no time to discuss controls and checks. One lab used palladium from Russia.

6.4 Vladimir TSAREV summarised Cold Fusion in Russia - there are many labs working and workshops have been held on it in Ekaterinburg and Donetsk. Many of the results sounded most impressive with claims of 500% excess heat and 100% reproduciblity but there was not time to determine the quality of the checks and calibrations and to understand which labs were using Russian palladium which could be heavily contaminated (according to Nate Hoffman). Vladimir is an excellent cartoonist and people particularly enjoyed a drawing of a lady in Japanese costume carrying a scroll on which the equation $E = mc2$ is scored out and instead is written $E = CF$. This was much appreciated by some who found it an excellent summary, while others just enjoyed it.

6.5 Dr F. SCARAMUZZI began by talking of the "strange geography of Cold Fusion". He said that in Japan, Russia, China and India there was a co-ordinated effort. In the USA there was a negative official position with exceptions (EPRI). In the EEC, it was the same except in Spain and Italy; what is still stranger is that behind the Alps, Cold Fusion never existed.

In Italy, the INFN, CNR and ENEA all fund Cold Fusion to a total of about $0.5 million (personnel not included). In the future it will be mainly INFN. He listed 7 groups (10 institutes) which are working on Cold Fusion. Most though not all are finding positive effects (he was one of the very few speakers to say that not everyone finds Cold Fusion effects - however it is a pity he did not mention the work of the Milano group of Ettore Fiorini who has the reputation of being one of the best and most careful experimentalist in Italy which is a country with a long tradition of excellent experimental work. He has performed one of the most complete and careful experiments looking for dd and pd fusion during electrolysis of palladium, plus mechanical straining to look for fractofusion. No excess heat was found and no gammas, neutrons, helium nor tritium - this in a very low background lab.).

7. CONCLUDING SESSION - ROUND TABLE

MCKUBRE, FLEISCHMANN, YAMAGUCHI, PERNG, TAKAHASHI, JONES, HAGELSTEIN, HANSEN; Followed by comments from the audience.

The members of the Round Table were each asked to talk for a short time.

7.1 Mike McKUBRE said that the 3C's of Cold Fusion were Collaboration, Co-operation and Correlation. After three and a half years there was no excuse for working on a single variable. All of experiments should be addressed and a correlation matrix established. The Harwell work which gave a null result, had correlations, we can similarly get information. The most interesting result is the correlation between excess power and D/Pd loading - as the loading increases the excess power increases steeply.

We have to understand the role of light elements.

7.2 Martin FLEISCHMANN said most people would like to see excess heat, but we say "No mystery". You must cram the deuterium in the lattice, let the temperature rise and then get excess heat. There are three things to do - (1) link material properties, (2) link electrochemical variables, and (3) do more work.

The Harwell experiment is a rich source of un-evaluated data.

We will make great strides in the coming year.

7.3 Dr. E. YAMAGUCHI said the helium production was very clear in his experiment and everyone should investigate, in situ, by real-time methods. He claimed that they clearly saw charged particle emission. They cannot say if the temperature rise is correlated with 4He production.

With hydrogen there was no 4He rise and no tritium but (and he said the data was not shown on Saturday) hydrated palladium did also give excess heat ie with ORDINARY hydrogen.

7.4 Dr. T.P. PERNG (ROC) talked of materials and hydrogen behaviour.

7.5 Dr. A. TAKAHASHI spoke of the need to correlate the excess heat and nuclear products - it was important to find out if there was a relation or not. He gave a list of which labs had found what (he seemed to mainly mention 9 labs except to say that many had observed neutrons - this list was much shorter than others such as that of Ed Storms; also it was noticeable that he did not give any numbers or rates to see if the various experiments agreed; also he did not talk of the more numerous experiments that did not find any effect, nor did he quote upper limits from these null experiments).

7.6 Steve JONES said there was one form of Cold Fusion that was irrefutable - Muon Catalysed Fusion. Since 1982 it has been known that the yield depends on temperature. The yield had been found to be greater than expected - 150 fusions per muon; it took 8 years before this was finally accepted.

For Cold Fusion they would continue to look for a low-level trigger. This they thought they had found - it is cement.

Somoluminesence involves the collapse of a bubble and gives a temperature of a million degrees and a megabar pressure - he now calls it somofusion. This might be of interest for Cold Fusion.

7.7 Tulio BRESSANI said one should relate energy measurements and neutron spectra - one expects a neutron of 2.5 MeV. Takahashi finds 4 to 6 MeV neutrons as well. Their own group has observed 2.5 MeV neutrons and has some indication of something in the 4 to 6 MeV region though their counters have lower efficiency there.

7.8 Peter HAGELSTEIN emphasized the strong relationship between theory and experiment - he had found this out when working on X-ray lasers. While he accepted heat from Pd/D in LiOD, did not feel the same way about Ni/H system in K2CO3. He said he works in Theory but often hears "This person should not be funded as he works on Cold Fusion".

On his personal wish-list, he would like;

(1) the 6Li to 7Li ratio be measured
(2) to know the value of the energy change in going from tetrahedral to octahedral positions in palladium
(3) the measurement of radioactivity in the palladium after a Fleischmann and Pons experiment.

7.9 Dr. L. HANSEN of BYU said that while energy was on one side of the equation, there must also be molten ash. This was a criteria to judge measurements of excess heat.

7.10 DISCUSSION

The Chairman, Dr. H. Ikegami invited comments from the audience. Nate HOFFMAN noted that one should be aware of what critics think. There are four artifacts that we should pay attention to;

1) A major problem. Helium diffuses through glass. Any glass in an apparatus has 4He in it and this can lead to false readings
2) gammas in Cosmic rays can give photo-disintegration of deuterium which

can give neutrons
3) radon decay products can be very troublesome, giving 8 MeV alphas, also 210Pb gives a 18 keV beta which can be mistaken for a tritium decay.
4) there is liable to be some radioactive palladium soon on the market place as palladium is being extracted from Russian reactors. Hence must take care and measure the radioactivity of Pd BEFORE the Cold Fusion experiment is done.

Comments were then invited from the floor.

Robert BUSH stated that there was very strong evidence for transmutation of light elements in water (ie ORDINARY water). In one year overwhelming evidence. Later in answer to a question, he said that his light water work was in a closed cell.

Dr. CHUBB said that there was a lack of internal review, especially of light water work. It is necessary to have outside observers as credibility is important. The loading should be given.

Steve JONES announced that they are setting up to do an experiment (in D2O) with picosecond timing.

The Conference Chairman, Dr. IKEGAMI asked for futher comments - silence. So everyone slowly got up and prepared to go. However after a while the Chairman called the meeting to order again. He thanked people for their presence at such an exciting meeting where we were informed that reproducible and controllable Cold Fusion had been observed. Especial thanks to Drs. Fleischmann and Pons and to Drs. Yamaguchi and Nishioka who had new and remarkable results. He said we are working for the future generation of energy in the 21st century.

He said the International Advisory Committee had decide that the next Conference would be in Hawaii.

The meeting closed with half-hearted applause.

8. NEXT COLD FUSION CONFERENCE - SCIENTIFIC MEETING

The Third Cold Fusion conference was sponsored by several respectable scientific organisations who have a long tradition of free and balanced scientific debate. After more that three years since the 1989 Fleischmann and Pons press conference, it was well known that the majority of the World's scientists did not believe in Cold Fusion and that there were many null experiments. It was to have been expected that the Organising Committee and the International Scientific Advisory Committee would have known this and when inviting speakers, would have chosen a balance. But only one sceptic was invited (Dr. Fukai was invited as a technical expert and it was a surprise when he reported that Cold Fusion should not work from the accumulated knowledge of many experiments). The token sceptic, who has never hidden his conclusions, was apparently expected to advise on how to perform future experiments (though the abstract also said that the experimental results will be reviewed).

In a normal scientific conference, more sceptics should have been invited to join the International Advisory Committee and then invited to speak at the conference. And when the token sceptic spoke, an orderly discussion should have followed. Instead of that for the Conference Chairman to take over from the session chairmen, then insult the invited speaker and close the session without any scientific discussion, cannot be considered normal scientific behaviour. It must have come as a surprise to the scientific societies that sponsored the conference.

It was announced that a Fourth Cold Fusion conference will be held in Hawaii in 1994. Will this be a scientific conference? Will it be sponsored by any scientific society that believes in free and balanced debate?
It is unlikely to be sponsored by the University of Hawaii as the University which initially took some responsibilty for the patents based on the Cold Fusion claims of some of their employees, organised a committee to investigate these claims and has now given up their interest in these patents.

10 CONCLUSIONS

(1) Overall there were fewer presentations of positive results than in previous annual conferences. This confirms the statistics on published papers.

(2) Many of the positive results tended to be "exotic" and different from the original Fleichmann and Pons and Jones techniques which were simple and "passive" unlike the present tendency towards "active" methods such as sharply varying the voltage or temperature.

(3) The biggest result was that some five groups claimed that positive effects were now being observed with LIGHT water. This was a shock as previously the justification that fusion was being observed was that the positive effect was observed with deuterium and NOT with hydrogen. However this comment was not made by anyone other than myself, and I had no response.

(4) Some of those claiming fusion with light water also claimed to have observed transmutation - the alchemists dream!

(5) The two experiments which started all the Cold Fusion effect, have both been very seriously put in doubt. A GE group with Fritz Will, the former Director of the Utah Cold Fusion Institute, found no effects in extensive attempts to repeat the experiments. Further checked the calculations (non-linear regression analysis with kalman filtering) and found that they had major problems and had not proved excess heat existed. Also the original experiment of Jones et al. is contradicted by the Kamiokande experiment. Thus both the foundation experiments are unreliable.

(6) The Takahashi et al. experiment which was welcomed and advertised, cannot now repeat the original levels of the effect claimed (this often happens to Cold Fusion groups, eg Huggins). Also he has the unique result that the yield of neutrons goes down as the excess heat increases.

(7) The NTT - Yamaguchi experiment was pre-announced by a press conference before it was presented for scientific discussion and evaluation at a conference - a procedure that is generally criticised. Afterwards there were serious criticisms about glass in the apparatus and the method of measuring excess heat. Further it was later announced that excess heat was also obtained with light hydrogen.

(8) The incredible 8 billion dollar movement in the NTT share value showed the powerful attraction of the dream of Cold Fusion. However the reality, the numbers, have to be looked at. After three and a half years the present claims of Cold Fusion are not substantially greater than in March 1989. And the majority of experiments find no excess heat. Further the better the quality and care of the experiments, the smaller the proportion that make claims. Further as Dr. Fukai showed, the thousands of experiments on deuterium and hydrogen in metals are against Cold Fusion.

(9) There is a major contradiction between the excess heat claimed of the order of Watts, and power calculated from the nuclear products observed. This is a question of factors of millions or billions or millions of millions - completely incompatible. If the basic source of the energy is the conversion of mass to energy, then there must be some nuclear products, but no Believer has solved this problem. This alone is a major reason for concluding that there is no fusion. Some believers in the existence of excess heat then say it is not a nuclear process, but then what could it be that would be of any practical interest?

(10) Many Believers in Cold Fusion genuinely want the Annual Cold Fusion conference to be a normal scientific meeting. But with the choice of speakers and rules, they have not been. This Nagoya meeting made it obvious to all that the Annual meeting is not scientific.

(11) The Regionalisation of Results (CERN/PPE 90-159, 1990) is stronger than ever and was described by Dr. Scaramuzzi to the embarrassment of the audience, but without protest.

(12) The overall funding of Cold Fusion is increasing. The previously known funding is decreasing and only INFN and EPRI are continuing appreciably. EPRI (US Electrical Power Research Institute) funding is partly used in the US and makes serious contributions to certain countries abroad, especially to Russia, China, etc. Figures of $3 to $12 million have been advanced but it

is seldom clear over how many years this is. At the Nagoya meeting, one became aware of major Japanese funding from industry, especially Toyota and next year MITI may invest some $3 million, but it comes under the umbrella of "Hydrogen Energy Research".

(13) In Japan the two most careful experiments have both given strong evidence that Cold Fusion will not give excess heat. They are the KEK experiment which was rather complete, and the Kamiokande experiment.

(14) It is sometimes said that if Cold Fusion had a one percent chance of giving excess heat that would be useful for power generation, then it should be studied. But the experimental results from Kamiokande show that this number is not one percent but is one hundred million millionth of one percent.

(15) If one takes all the factors, experiments, theories etc. together, the balance of evidence is strongly against the existence of Cold Fusion. Having looked at the evidence for and against, more than 99% of the World's scientists do not believe that Cold Fusion could give useful energy.

NOTES.

i) This is a long review with probably well over a thousand pieces of information so there must be some mistakes. Will be pleased to receive corrections.

From experience expect there will be some propagandists who will use the technique employed by a few unscrupulous lawyers, of taking one error and saying that hence all must be false. Scientists on the other hand, try and take ALL data and theories and try and make sense of them - and as Dick Feynman would point out, it is sometimes necessary to make sense of all the available information, to assume that some experiments are mistaken. However doubt if a few errors will change the overall impression of the conference which was of a winding down with fewer new results than in previous conferences, an increase of propaganda and an increase of regional funding plus some extraordinary results, some of which (fusion in ORDINARY water), contradicted previous work, plus some cranks. Also some errors will not change the impression that this was not organised to be a normal scientific conference since no serious attempt was made to report the many experimental results which have made the majority of scientists disbelieve in Cold Fusion.

ii) In a note it is not possible to report everything - please ask the people named for further details.

iii) CURIOUS STORY. In an early partial version, a curious story was added describing how a demonstration had been set up by Dr. Notoya of Hokkaido on a table just outside the conference room. It was said to show two identical open cells with ORDINARY water but one with K_2CO_3 and nickel cathode, and this latter cell was much hotter to the touch than the calibration cell. This was claimed to show Cold Fusion with ordinary water. However David Buehler, a student of Steve Jones, noticed that the electrical leads were not identical, the one to the control cell was much thinner so that its resistance was higher and energy was dissipated in the thin wire and not in the control cell as advertised. He checked by moving the clip.

He and Steve were savagely attacked (as usual!), but Steve showed from his log-book that the effect was serious and then later after further exchanges, they repeated the experiment in BYU based on these numbers, and showed a 10 degree temperature difference.

Dr. Notoya will be visiting the States and is going to repeat her demonstration at MIT on 4 December and it is said by her propagandist that it will work, later he said it might not. Have the impression that some will try and concentrate on the size of the wires which are sure to be the same this time. However this is a red herring. The real problems are two-fold;
a) one of the voltages is 1.48 Volts higher to compensate for electrochemical effects - but Tom Droege has already found that this number of 1.48 V is not safe and others have also shown this recently. So this value of 1.48 V has to be established first
b) only do good calorimetry with closed cells and several constant temperature

baths surrounding the cell. (It has been said one needs to do a non-linear regression analysis to obtain a result with such an open cell!)

OTHER NEWS

A).The Wall Street Journal of 27 November reported that NTT is selling a kit containing all instructions and equipment needed to replicate the Yamaguchi and Nishioka experiment. The price is $565 000 and it is obtainable from Advanced Film Technology INC which is 51% owned by NTT. Steve Jones says the W.St.J. quotes the NTT President, Masashi Kojima, as saying that "the result will likely be a Nobel prize for Mr. Yamaguchi" if another scientist replicates Yamaguchi's experiment, and says that NTT might "become a power company based on cold fusion", quoting the NTT President. Have just checked the NTT share price at the time of this announcement - there was no billion-dollar jump in the share price this time.

B). Frank Close has been following up the way in which a first graph of Fleischmann and Pons showing a peak at 2.5 MeV moved to 2.2 MeV. He notes that this was after a talk by Martin at Harwell on March 28th, when he was told that while the neutrons should emerge with an expected energy of 2.5 MeV, they should be slowed down to thermal energies before being captured, and hence the peak should be at the lower value of 2.2 MeV. Frank says that at 09.32 on the 30th March a Fax was sent from the University of Utah Chemistry Department making the change.

The graph was also changed in that the bin size switched from 100 to 200 keV, but the shape of the distribution of data points on the graph did not change. Fleischmann has written that this was a change caused by going from a linear to a quadratic interpolation - but this makes no mathematical sense.

A further change was that the number of counts jumped by a factor of nine.

It is hard to see how these three changes from one graph to the other, could be covered by patent secrecy. No doubt the judge in the La Repubblica trial would like to study the documents.

In reply to a recent letter from Frank to Martin, a letter has been received from Mr. Triggs, the laywer of Stan Pons. He says that pending patent applications, all documents relating to work in Utah are prime source materials and are confidential. He warns Frank about the documents he has and says that there were thefts from his clients' laboratory. Now this is a serious criminal matter and it would be interesting to see the reports of the University authorities and Police on these thefts - these documents would presumably not be covered by patent problems. It should be noted that Frank has no intention of revealing any sources or information which are not already in the public record.

C) The Fleischmann and Pons paper mentioned in section 3.6, firstly describes new measurements they have performed using a high resolution, but low sensitivity (efficiency) Germanium detector. One of the points they wish to make is that this is better than a low resolution, high efficiency detector as used by those who found nothing. However their new Ge detector efficiency is only 2 E-5 which is not so different from their old BF3 detector (dosimeter) which was 2.4 E-6 (this why their old counting rate was so low even though they claimed 40 000 neutrons per second after correcting for efficiency). The gamma ray spectra they present show a smooth background with some very sharp resolved peaks and there is a large sharp peak near 2.2 MeV where one expects a peak from capture of slow neutrons, the actual value being 2.224 MeV. It takes a minute to realise (and one is not told till much later) that this splendid peak is background from 214Bi at 2.204 MeV and the miserable little bump to the right of it, is the peak at 2.224 MeV - the relative peak heights is 19 to 1.

Now there are neutrons everywhere, from cosmic rays, from the plaster, concrete etc. so there should be a peak at 2.224 MeV especially as the experiment has not been done deep underground nor is there special shielding. So the question is how was the normal background measured? There is no

description in the paper of the measurement of this unavoidable background - so it is possible that this small peak is 100% background. However there are two measurements reported AFTER the current was switched off and these are said to extend to two diffusional lifetimes, so it is tempting to consider these as background measurements - and since small peaks are seen at 2.224 MeV of about the same height as the ones observed, one would normally conclude that this shows that there are no extra neutrons coming from Cold Fusion in addition to the unavoidable background. However such is not the conclusion of Fleischmann and Pons who instead conclude that this is an interesting and significant effect lasting up to 30 days after the current was switched off. Why did they not calibrate BEFORE the experiment began?

They claim a rate of 5 to 50 neutrons per second per Watt which they note is less than their previous value of 4000 neutrons per second (the 1989 paper says 40 000 neutrons per second). They do not see this as a discrepancy, but claim this must be due to them under-estimating the sensitivity of the previous instrumentation (ie by several orders of magnitude).

They claim that previous works, Petrasso et al. and Salamon et al. were insensitive because with their poorer resolution, they would not have been able to see the 2.224 MeV peak because it would be buried in the 2.204 MeV which would now be wide - and they present a graph to illustrate this. Now if the efficiency of these two experiments was as poor (2 E-5) as that of Fleischmann and Pons, this would be true. But it is not true, because their efficiency was very much higher so that for the suggested neutron rate, their peak would have been much bigger than the 214 Bi peak at 2.204 MeV and been clearly visible. To give some numbers, if their efficiency was as low as 2% which is 1000 times more than F&P's, their peak would have been 1000 times bigger and this would have been 50 times bigger than the 214 Bi peak at 2.204 MeV.

The conclusion is that the paper, as presented, gives no compelling evidence of any neutrons from the Cold Fusion cells.

Overall the measurement of neutrons at fairly low counting rates is not easy as many have learnt, and it is best left to experts.

HAVE A NICE TOMORROW
(this delightful phrase was seen in a Takayama shop window).

cold fusion

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REVIEW OF COLD FUSION EXPERIMENTS IN KAMIOKANDE WITH TITANIUM AND PALLADIUM

Douglas R.O. Morrison.

ABSTRACT

Jones et al. and Menlove et al. have claimed production by Cold Fusion, of two types of bursts of neutrons, one lasting some hours and the other some hundreds of microseconds. These experiments involving the penetration of deuterium into palladium or titanium by gas pressure and by electrolysis, have been repeated inside the 3000 ton Kamiokande detector. It is shown that with the very low background levels attainable in Kamiokande, neither of these two types of bursts of neutrons are observed and the upper limits are so low that the previous claims are disproved.

INTRODUCTION

A brief account of Kamiokande experiments on Cold Fusion using titanium and palladium was given in a review of Cold Fusion given at the Third International Cold Fusion Conference held in Nagoya, October 1992. A more complete account is given here.

Kamiokande is a detector consisting of 3000 tons of highly purified water deep underground. The walls are covered with large photomultipliers which can observe Cherenkov light from charged tracks. The basic idea is that Cold Fusion cells be surrounded by a brine solution and put in the centre of the Kamiokande detector. Any neutron from the cell has a good chance of being slowed and absorbed by the chlorine in the brine to give an excited isotope which decays by emitting an energetic electron which can give Cherenkov light.

The Kamiokande experiment consisted of three parts. Was told that initially only experiments using electrolysis with titanium and palladium cathodes and gas loading of deuterium in titanium were foreseen. These appeared to give no cold fusion effects so thirdly, gas loading of cement was tried (to follow up the hypothesis that "Mother Earth" gives Cold Fusion) and effects were found. The two earlier experiments were re-analysed and some effects were found.

These three experiments are rather fully described in the thesis of Taku Isheda (ICRR Report -277 -92-15 which deserves reading because of its clearness and great detail). Figures referred to below are from this thesis.

In my review of the Nagoya conference, Part 4, Saturday 24 October, concerns talks by Jones, Yamaguchi, Miles, Iida, Kasagi, Cecil, Tsarev, Gozzi and Morrison. Thus 4.1 was Jones - who talked of several subjects and described the results of the Kamiokande experiments using cement, but did not describe the two earlier experiments using electrolysis or gas loading of Pd and Ti. Section 4.9 describes the review by Morrison; most of this talk was a general review, plus the question of the two foundation experiments presented in March 1989 by Fleischmann and Pons(press conference) and by Jones et al. (word-of-mouth and BYU report). They are discussed as these two experiments, which started all the Cold Fusion excitement, are now severely challenged. He described the repeat in Kamiokande of the earlier experiments of Jones et al.

and of Menlove et al. which extended the original work.

The March 1989 paper of Jones et al.[1] described 14 runs, one of which, number 6, gave a neutron yield higher than background which lasted about 7 hours and the Nature paper[1] quoted a rate of 0.4 neutrons per second. It was claimed that this was a five standard deviation effect. Note the figure in Nature is most unusual as for clarity of presentation, 10 counts were added to each bin, thus 0 counts became 10 counts, one count became 11 counts, etc.

Subsequently this rate was lowered by taking the average rate in a different way - by averaging over all the 14 runs, this gave a rate of 0.06 neutrons per second.

Thus two rates are quoted;
A - the rate during the burst - 0.4 n/s
B - rate averaged over all time - 0.06 n/s.
(note this would imply that the total time for 14 runs was about 47 hours).

Later an experiment was performed jointly with Howard Menlove[2] and two examples of enhancement of the neutron rate were observed for 17 and for 5 hours in a total running time of 1703 hours[3]. This is the basis of the claim that this "random" neutron emission occurs every 850 hours, this later was given as 900 hours when all runs were considered. Also a new phenomenon was observed, in a time interval of 128 microseconds, short bursts of neutrons were observed.

Thus now two phenomenon are claimed - short bursts and long bursts (the long bursts are named "random" by some, but a better description is that which was used in the original paper - "few-hour burst"). Thus the claims are for two types of bursts, one of the order of 100 microseconds and the other lasting hours. These two effects differ by more than a factor of ten million in duration. It is surprising that have not seen anyone make this comment before - an interesting challenge to theoretians to explain this factor of ten million.

These two effects, few-hour bursts and micro-bursts, will be considered in turn.

FEW-HOUR BURSTS

Three few-hour bursts have been claimed, lasting 7, 17 and 5 hours. During the bursts the rate, A, was 0.4 n/s for the first[1] with electrolysis with a palladium cathode and 0.05 to 0.2 n/s for the other two[2] with gas-loaded titanium. These latter few-hour bursts were just above the background (about 10% in fig. 1-3e). The background for the 17-hour burst was 0.39 counts/second and the values during the few-hour burst go from 0.405 to 0.46 counts/second - they fluctuate outside statistics. The 5-hour burst was at a rate of 0.26 counts/s above a background of 0.24 c/s.

Thus it can be seen that the few-hour bursts are very small effects just above background. Also the power claimed is extremely small, about 10 E-13 Watts - this led Steve Jones to state from the beginning in 1989, that his observations are in serious disagreement with the Fleischmann and Pons observations. They indicate no hope of Cold Fusion giving useful energy.

With Kamiokande having a background of about 100 to 1000 times lower than 0.05 to 0.2 n/s (see fig. 4-11), it was expected that the few-hour bursts would be spectacularly above background.

The results from Kamiokande are summarized at the beginning of Chapter 6, "Random neutron emission(ie few-hour bursts) beyond the background level has not been observed both for the cylinders (ie gas) and from the electrolysis samples" (this excludes the cement results and runs done with both electrolytic and cement cells in place at the same time). The numbers are;

Pressurized D2 gas;
Flux upper limit = 0.00008 neutrons/second at 90% confidence limit.
Total live time = 1310.7 hours.

Electrolytic Cells
April set, Flux upper limit = 0.000098 n/s at 90% CL
Total live time = 387.2 hours.
July set Flux upper limit = 0.000057 n/s at 90% CL

Total live time = 569.7 hours.

Now it might be felt that this was rather strong evidence that these results were in contradiction with the earlier results of Jones et al. who had quoted 0.06 n/s for type B rates that is for the total time and this is correct - the Kamiokande results are in severe disagreement with Jones et al.

But as indicated in Ishida's thesis, there is a possible problem. This is that Jones and Menlove claim that few-hour bursts occur only once in 900 hours. For Menlove alone the numbers[3] are 2 bursts of 17 and 5 hours in 1703 hours of running indicating one few-hour burst per 851 hours (have not been able to find the numbers for the original Jones et al.[1] paper, but if they found one burst, then that would have to have been over 1000 hours to obtain a combined average of once per 900 hours? - on the other hand 47 hours was calculated above?). Now the total Kamiokande running time is
1310.7 + 387.2 + 569.7 = 2267.4 hours
This is only 2.5 times 900 hours which is discouraging for few-hour emission but not significant. Though how can the Kamiokande results be in severe disagreement with Jones et al.[1] but not with Jones[1] and Menlove[2] taken together?

However there is a crucial factor that appears not to have been considered. This is that the masses of the metals used in the various experiments were very different and a correction should be used for that.

In the Jones et al. experiment the mass of titanium was only 3 grams. In ref. 3, the average mass is given as 84 grams. Combining this one obtains for the three few-hour bursts, about 57 grams average per few-hour burst.

This is to be compared with the masses used in Kamiokande. For the pressurised gas samples, they vary from 150 to 1700 grams, the average being 339 grams. Thus the 1310.7 hours should be multiplied by the factor of (339 divided by 53 =) 6.4 giving 8379 hours. This is now significantly greater than the 900 hours per burst claimed.

The conclusion is that the Kamiokande results for gas -loading alone do give a statistically significant result and invalidate the few-hour burst claims of Jones et al.[1] (electrolysis) and Menlove et al.[2] (D2 gas).

The masses of the titanium and palladium samples used for electrolysis are not given directly in Ishida's thesis but the volumes are given. Since no few-hour burst were observed in Kamiokande with electrolysis, this reinforces the conclusion that the balance of evidence is against the existence of few-hour bursts.

A consequence of this is that all the calculations based on 0.4 n/s, about the possibility of there being Cold Fusion in the Earth etc. given in the paper to Nature, are also inappropriate by several orders of magnitude.

MICRO-BURSTS

Menlove et al.[2] claimed that they had observed bursts of neutrons in a gate of 128 microseconds. In fig 1-3b typical bursts of up to 60 neutrons can be seen (elsewhere up to 85 neutrons in a burst) while the background only goes up to bursts of about 5 neutrons. These are observed numbers of neutrons; after correcting for efficiency, the number of neutrons in the burst coming from the source can be calculated. In fig. 1-3(c) is shown the distribution of source neutron multiplicities. It extends from 10 to 280 (the interval 0 to 10 is not taken as the background is too high). There are a total of 69 bursts of which 31 are in the bin 10 to 20 neutrons, 14 between 20 and 50, 12 between 50 and 100 and 12 above 100. It is said on page 4 that Menlove et al. ran for 9995 hours (note - this is for micro-bursts, for few-hour bursts it seems that the running time was only 1703 hours)

This experiment was repeated in Kamiokande. As the background was so much lower, the gate was extended from 128 to 500 microseconds which improved statistics slightly. Following Menlove et al., the cells were cooled to liquid nitrogen temperature and then warmed up expecting that as previously (and following the Frascatti results) that neutron emission would occur between -50 and -10 C. A large volume was chosen so the neutron detection

efficiency was high - about 37.3%. Thus if 4 neutrons were observed, it was taken that this corresponded to 10.6 source neutrons and if 3 neutrons were observed this corresponded to about 8 source neutrons[illegible]section 6-1-2[illegible]).

A. RESULTS FROM GAS LOADING

19 samples containing 87 cells, were run for 1305.3 hours with D2 gas loading. One sample was run with H2 gas loading.

5 bursts were found, 3 of multiplicity two and 2 of multiplicity three. This implies there was no evidence for any bursts with multiplicity greater than ten whereas for the smallest multiplicity bin. from 10 to 20, Menlove et al. found 31 in 9995 hours, ie one per 322 hours, so in 1305 hours 4.2 bursts were expected and none were found. This is ignoring the fact that the masses of the samples were quite different. The Menlove et al. cells averaged 84 grams while the cells placed in Kamiokande averaged 339 grams. Thus a correction factor of 339/84 = 4.0 should be applied. Thus from the results of Menlove et al., one would expect 4.2 times 4.0 = 16.8 bursts with source multiplicity between 10 and 20 and zero were observed. It must be concluded that the Kamiokande results are in serious disagreement with the Menlove et al. bursts results - this is for the lowest multiplicities that Menlove et al. could measure above their higher background, that is, of 10 to 20 source neutrons.

If one takes all multiplicities, Menlove et al. found 69 bursts with source multiplicity above 10 in 9995 hours. Hence Kamiokande in 1305 hours would be expected to have observed 9.3 bursts but observed none. If one corects for the different average masses, then one would have expected 37 bursts and none were observed - more exactly none were observed with a multiplicity greater than three (since 3 observed neutrons corresponds to 8 source neutrons).

The samples were cooled to liquid nitrogen temperature and warmed up. Previously Menlove et al. claimed bursts in the region -50 to -10 C (as did Frascatti) but as shown in fig. 6-4, none of the bursts occurred at such temperatures - as Dr. Ishida writes on page 51, this clearly conflicts with the results of ref. 2.

Of the 19 samples, one sample (K-4) had four bursts, one had one burst and the other 17 samples had no bursts.

The overall conclusion is that the Kamiokande results with D2 gas filling, are in disagreement with all the results of Menlove et al. also made with gas filling.

B RESULTS WITH ELECTROLYSIS.

Experiments were run for 1046.2 hours with Deuterium and 2617 hours with Hydrogen as background runs. With deuterium 9 bursts were found with the multiplicities of; 5 of multiplicity two, 2 of mult. three and 2 of mult. four. With the hydrogen, 6 bursts were found; 5 had multiplicity two and 1 had mult. three.

It has been claimed that the rate of bursts at low multiplicities is consistent with that observed by Menlove et al. in their experiments with gas loading but different for higher multiplicities. The former claim will be examined;

By lowest multiplicities is meant the 10 to 20 bin of source neutrons and only the two observed bursts of multiplicity four are concerned(there is a second order effect if the spread of multiplicities is considered, but doing a full correct calculation will not change the numbers in a significant way). Thus Menlove et al. would predict one burst in this 10 to 20 source mult. bin in 322 hours and hence 3.2 in 1044 hours - and this is consistent with the 2 observed in Kamiokande. Again one should correct for the different masses of the samples. However this claim of agreement ignores that the fact that the Menlove et al. experiment was done with gas loading; and the gas loading results in Kamiokande are inconsistent with Menlove et al. So combining the two experiments, gas plus electrolysis, there is no agreement for the lowest multiplicities.

Further if the Menlove et al. results were correct, then the very low

background experiment of Kamiokande should have shown very dramatically the other multiplicities - the other low multiplicities from 20 to 50 where 14 bursts were claimed, but none found in Kamiokande, and the 22 bursts claimed by Menlove et al. with multiplicities between 50 and 280 but where again, none were observed in Kamiokande.

There is the question of what was Kamiokande observing with the 5 + 9 + 6 events which were recorded with multiplicity greater than two. This question is quite separate from the question of whether the Kamiokande results are consistent with Menlove et al. results - that question has been settled.

One obvious explanation is fission of contamination in the cells, for example, fission by uranium. In Dr. Ishida's thesis this is considered and two comments are made;

a) from calculations, the burst distribution for uranium for multiplicities of 0, 1, 2, 3, and 4 observed neutrons, is expected to be 25.1%, 53.7%, 17.8%, 3.1% and 0.3% resp. Now multiplicities zero and one cannot easily be measured but the multiplicites for 2, 3 and 4 neutrons looks slightly different, there being more of multiplicities 3 and 4. So is this a new effect or is there possibly an explanation that has not been considered?

b) Is there enough uranium in the titanium and in the palladium to account for the effects observed? On page 51 it is noted that in sample K-4, 4 bursts are observed in 608.4 hours and this would correspond to 1.3 +/- 0.7 milligrams of uranium. Sample K-4 had 300 grams of Titanium - this would then give (1.3 +/- 0.7 mg)/300 g = 4.5 +/- 2.3 ppm of U in Ti. The uranium background was not measured in the cylinders (ie 300 g titanium and 2000 grams of holder) but has been measured elsewhere when less than 1 ppm was found. This might seem to settle the question and say it could not be uranium. However there are two important factors neglected;

i) Only sample K-4 was taken out of the 19 samples with gas loading and the 16 samples with electrolysis. And of the 5 + 9 bursts recorded, no less than 4 were found in sample K-4. A full calculation taking all 19 + 16 = 35 samples plus the 5 + 9 = 14 bursts, would reduce the ppm by a factor of ten and bring the two numbers into approximative agreement.

ii) Kamiokande measures neutrons occurring in the water outside the brine and the cells. It does not measure neutrons coming from the cells alone. Now when there is fission, gammas as well as neutrons are emitted almost simultaneously. These gammas can create neutrons by photo-disintegration. Hence the number of neutrons created by the fission plus photdisintegration in the cell combined, is higher than that from neutrons alone. Hence the table of multiplicities given above from calculation, needs correcting and this correction will increase the higher multiplicities and also decrease the amount of uranium needed.

A further important point is that it is unsafe to use hydrogen as a background for deuterium because the gammas do cause photo-disintegration of the deuterium to give additional neutrons but do not give additional neutrons with hydrogen.

Thus the choice of brine to detect neutrons in the special circumstances of Kamiokande, does cause special problems. A better solution for a background run, would be to place the cell in Kamiokande but not to switch on any current.

OVERALL CONCLUSION

The overall conclusion is that all the results obtained by Jones et al. and by Menlove et al., are disproved by the Kamiokande experiment which has much lower background and hence much lower limits.

Acknowledgements:

Thanks are due to Steven Jones for comments and for answering many of my questions - however this does not mean he necessarily agrees with all the contents and conclusions. Again the scientific community must thank the Kamiokande group who for the third time have performed a crucial experiment in a field, the others being on Supernova neutrinos and Solar neutrinos.

References:
[1] S.E. Jones et al, Nature 338(1989)737
[2] H.O. Menlove et al. J. of Fusion Energy, 9(1990) 4
[3] A.A.Anderson and S.E. Jones, AIP conf. Proc. 228, "Anomalous Nuclear Effects in Deuterium/Solid Systems, Eds. S.E. Jones, F. Scarramuzzi and D. Worledge,AIP, New York, p 24 1990.

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TO: Dr. Richard Garwin FROM: Dr. Scott Chubb

NO. OF PAGES: 27 (INCLUDING COVER SHEET)

IF YOU DO NOT RECEIVE LEGIBLE PAGE(S), PLEASE CALL:

COMMERCIAL: (202) 767-2003 AUTOVON: 297-2003

REMARKS: I am including a cover letter that provides the additional information concerning how to obtain Como Proceedings, my phone number, etc.

NRL/DIPL

25 Nov. 1992

Dr. Richard Garwin
IBM Thomas J. Watson Research Center
P. O. Box 218
Yorktown Heights, NY 10598

Dear Dr. Garwin,

As I mentioned to you over the phone, I am forwarding a copy of Michael McKubre's paper from the Como Proceedings, a copy of the abstract that Kunimatsu presented in Japan, and information, contained in this letter, concerning how to obtain a copy of the Proceedings from the Como conference. This Proceedings is not widely available. It is worth emphasizing, at least in my mind, that the most important developments concerning reproducibility, system characterization, etc, associated with the excess heat, really have come out during and since the Como meeting.

The Conference Proceedings (for the Second Annual Cold Fusion Conference, held in Como) have been available through the Italian Physical Society since last February. The title of the Proceedings is:

Volume 33, "The Science of Cold Fusion Conference Proceedings."

The ISBN number of this book is 88-7794-045-x. The book is published by the "Società Italiana Di Fisica" (Italian Physical Society). This book may be purchased directly from the Società Italiana Di Fisica. If you purchase it and ask that the book be delivered by air mail, the cost is 110000 Lira (about $100). If you purchase the book and request that it be delivered by standard book-rate delivery, the cost is 90000 Lira. Probably the Società will take a check, drawn from an American bank, paid in dollars. (I paid for reprints for an article that we have in the Proceedings in this manner.) The address to write to to purchase this book is:

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Via Loderingo Andalò 2
1-40124, Bologna, Italy

The relevant phone number for making inquiries (in Europe) is (051) 58.15.69. The relevant FAX number is (051) 58.13.40.

Yours truly,
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22-P11-27

Deuterium loading ratio and excess heat generation during electrolysis of heavy water by a palladium cathode in a closed cell using a partiallly immersed fuel cell anode

Keiji Kunimatsu

IMRA JAPAN CO. LTD., Sapporo Japan

We have developed a nobel electrolytic cell pressurerized by deuterium gas in which deuterium loading ratio in a palladium cathode can be determind in-situ during the cold fusion experiments. A gas diffusion type fuel cell anode which was partially immersed in the electrolyte served as an anode to ionize deuterium gas to deuterium ions. The loading ratio can be determined from the deuterium gas pressure dectrease during the electrolysis at constant current. The temperature in the electrolyte, in the palladium cathode and in the gas phase were monitered simultaneously with other parameters such as pressure, cell voltage, cell current and hydrogen overvoltage on palladium measured against the reversible hydrogen electrode(RHE) in the same solution.

Fig. 1 and 2 demonstrate a result showing dependence of the loading ratio, D/Pd, on the hydrogen overvoltage which has been corrected for the Ohmic resistance of the electrolyte, and the excess heat generation with respect to the input electrolytic power as a function of D/Pd in 1M LiOD.

The excess heat generation becomes prominent around D/Pd =0.85. Currently we are trying to go into the region of the higher loading ratio.

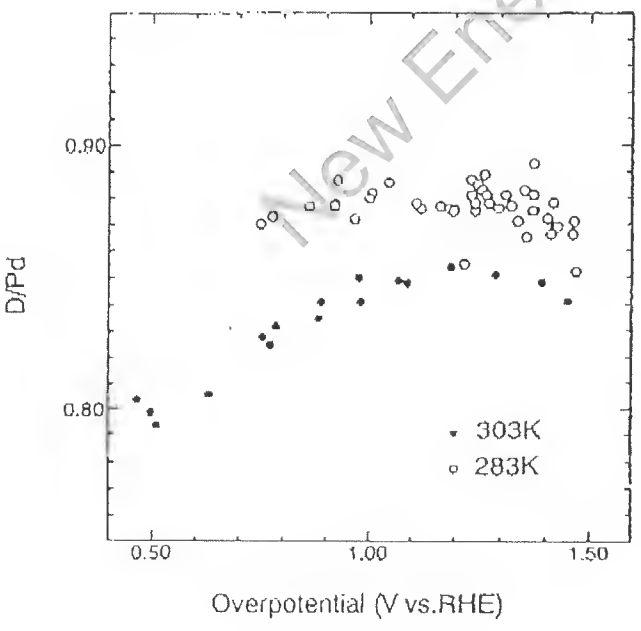

Fig. 1 Dependence of D/Pd on overvoltage

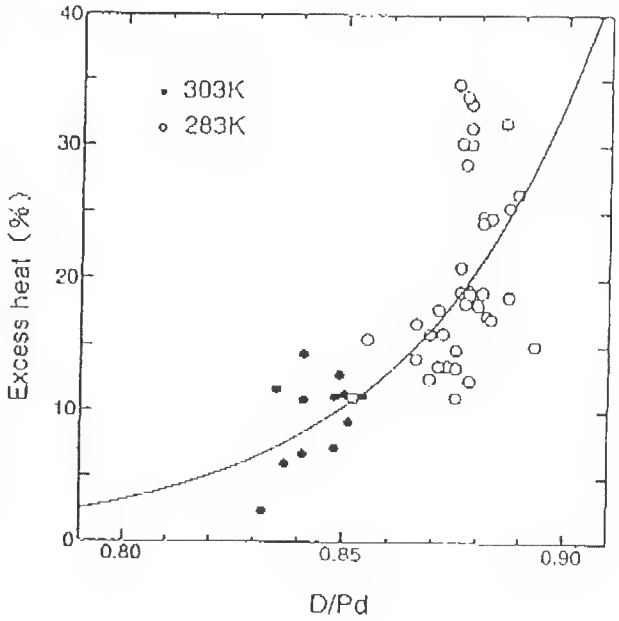

Fig. 2 Dependence of excess heat on D/Pd

MS: FC92.055

DRAFT: 03-18-92

Test-Tube Fusion:
the Loud Beginning

Frank Close

Frank Close is head of theoretical physics at Rutherford Appleton Laboratory in the United Kingdom.

When two chemists, Martin Fleischmann and Stanley Pons, announced on March 23, 1989, that they had produced nuclear fusion in a test tube at room temperature, reactions from nuclear physicists ranged from astonishment to skepticism.

Decades of study in nuclear physics suggested that Fleischmann's and Pons' claims were impossible. Moreover, if their test tube had generated watts of power from a nuclear process, billions of nuclear transmutations must have occurred each second and the radiation—especially the neutrons and gamma rays—should have left them in worse health than they seemed. Nonetheless, circumstances made it sensible to check their "findings."

First, there was a claim that an independent group, led by Steven Jones at Brigham Young University (only 50 miles from Pons' University of Utah laboratory in Salt Lake City) had performed similar experiments and observed nuclear radiation—specifically neutrons. Second, it seemed unlikely that someone with Fleischmann's reputation (he is a fellow of the Royal Society of London and a renowned electrochemist) would recklessly risk all with a claim that had not been well researched.

Announcements from Utah in the days following the initial press conference proclaimed that, in continuing experiments, power production had increased to eight watts from a one-watt input. In addition, the world witnessed the confident appearance of the scientists in Washington appealing for funding for commercial development of cold fusion.

The experiments were easy to set up and, it turned out, easy to misinterpret. All that one needed was a small cell containing heavy water and palladium metal, and power from a battery. Once the cell was charged with electricity, you could then measure the cell's heat output (technically power) and compare it with the battery's input. An excess—manifested, for example, by an unexpected rise in temperature—could be evidence for a new power production process, or for miscalibration.

If the former, the question remained whether this outcome was due to a chemical process or nuclear fusion. According to the chemists' calculations, the cell's net power production was too large to be caused by a chemical process; therefore, they concluded, it must be nuclear. Their claims to have sighted nuclear radiation—neutrons, gamma rays, and tritium—reinforced that impression, as did news of Jones' independent work.

Individuals and groups worldwide set up their own tests. Many were surprised to discover how complex the palladium/deuterium system could be and how rapidly temperature could rise when one sends electricity into the cell. Having been led to believe that the process was easy to produce and then

having seen a sudden temperature rise in their own experiment, many thought they too were witnessing cold fusion and called their own press conferences.

Today, these early claims generally have been dismissed. Some, who wish to believe that an "establishment conspiracy" has caused the world to ignore a great discovery, contend that focusing attention on the early discredited experiments has kept subsequent developments from being aired. Be that as it may, the early announcements, often reported by the media and not scrutinized by experienced eyes, are what led to the widespread belief that "something exciting is happening."

Had these incorrect "confirmations" not received such hype, or had the reality behind the original claims been more widely known, interest in test-tube fusion as a potential commercial power source would have died in a few days.

Desire and Reality

If one removes delusion and desire from the psychology of the experimenters, evidence shows there may have been a chemical process involved, or nothing significant, but there is no evidence, nor has there ever been, of watts from nuclear fusion.[1]

My research into the early history of cold fusion, and in particular the work of Fleischmann and Pons, shows that public perceptions and government reactions were driven by a lack of

awareness of the actual events. To put it simply, public presentations differed from reality, sometimes considerably.

My research spanned not only the United States, but also Europe, where I gained access to the records at the United Kingdom Atomic Energy Authority's Harwell Lab. Fleischmann, who served as a consultant to Harwell, advised the lab of his research before his March 1989 press conference. Harwell researchers began their own cold-fusion experiments weeks before the rest of the world learned of the sensational Utah claims. By June 1989, scientists at Hartwell were ready to state publicly, and publish in Nature, that they found no evidence to support Fleischmann's and Pons' claims.[2]

The essential idea of the cold-fusion experiment was to pass electric current into a small cell containing heavy water and palladium metal. The current would split the water into its constituents, the nuclei of deuterium atoms being forced into the palladium where, Fleischmann and Pons believed, the nuclei combined (fused) to produce new nuclear species and heat.

Minimum tests to establish a nuclear, rather than a chemical process, include:

- What happens when one replaces heavy water with ordinary water? If anomalous heat is associated only with heavy water, this could signal a nuclear process because heavy water contains the essential deuterium fuel. However, if there are comparable amounts of anomalous heat with heavy and light water, a chemical reaction is likely.

■ All chemical and nuclear reactions involve change. Chemical reactions involve atomic or molecular changes in which elements are not altered; nuclear reactions involve changes in the elements themselves.

Thus, conclusive evidence as to the nature of the process involves identifying the products of the reaction and checking to see if their amounts are commensurate with the power output. There has been a reluctance to carry through the first of these tests among those "true believers" who insist that a new nuclear process is at work. Varying the inputs and quantifying the heat response is another crucial test.

Reports on the latter from Fleischmann and Pons have changed in such self-contradictory ways as to strain their credibility and limit assessment of their work.

Fear that the University of Utah would lose potential patents to Jones' group pressured Fleischmann and Pons into a premature announcement. Between the fall 1988 and Easter 1989, Marvin Hawkins, a student at the university (joined at Christmas 1988 by Martin Fleischmann), measured the heat output of a few cells. The cells contained palladium rods with diameters ranging from 1 millimeter (mm) to 8mm. Some cells gave no net heat output. The chemists, however, decided they were "dead" cells—that is, missing a vital unknown ingredient.

At this stage, the chemists were intent on establishing qualitatively if there was a real phenomenon. According to their calculations, some cells (with rods no bigger than 4mm radius)

ran slightly hotter than expected. This convinced them that the process deserved additional study. Optimistic that the effect's magnitude would increase by increasing the volume of the palladium (thus giving promise of larger-scale exploitation), they ignored the failure of their larger, 8mm diameter rods, to give any net heat.

When they replaced palladium with platinum they found heat balance too (that is, no excess heat). This observation spurred them to believe that their calibrations were accurate and that the heat measured with palladium was due to the palladium's presence. This, in turn, heightened their conviction that fusion inside palladium was the key.

However, no control experiments replacing heavy water with ordinary water had been made before the press conference. Fleischmann made this clear when asked directly by Carlo Rubbia, director general of CERN on March 31, 1989—just a week after the initial news conference.

Rubbia: "Have you replaced deuterated water with ordinary water?"

Fleischmann: "I have to confess...that these experiments are just going on now and we will hope to give you that answer very shortly." He continued, "The reason we haven't done so is that if you do that experiment you ruin the electrodes and we have so far had very limited resources." This response was similar to the one made to a small gathering of scientists at Harwell on March 28.

It is difficult to reconcile these statements, made when memories were fresh, with a published assertion in 1991 that they had performed experiments with ordinary water before going public and, moreover, that the heat ledger in these experiments had balanced.[3] The key is Fleischmann's reply to Rubbia in which he mentions that experiments with ordinary water were in progress.

In Pons' opinion, those experiments produced heat.[4] On April 14, Pons told the former chairman of the U.S. Atomic Energy Commission, Glenn Seaborg, that the University of Utah chemistry department had come to the same conclusion. Furthermore, on April 9, Pons encouraged Charles Martin's team from Texas A&M to announce publicly the first (erroneous) replication of the "Fleischmann and Pons effect," based upon his (Pons) belief that ordinary water as well as heavy water generated heat.

Heat with ordinary water should have provided evidence that either there was an error (as turned out to be the case with the Texas experiment) or that a chemical reaction was occurring.

What happened appears to be a classic example of commitment to a prior belief causing blindness to the contrary evidence. Instead of drawing one of two logical conclusions, Pons chose a third: heat was due to fusion between protons (in the water) and deuterons (that are in traces of heavy water and that even occur naturally in ordinary water at about 1 part in 6,000).

To test this hypothesis, one would have to perform a series of experiments that varied the proportions of water and heavy water to see how the heat production varied. Because Fleischmann

had not yet conducted these experiments, he was "not prepared to answer" questions that raised this issue. He also may have been unwilling to answer because of patent implications. The experiments, if successful and reproducible on a large scale, could prove extremely lucrative to both the experimenters and the institution for whom they worked.

Pons' and Fleishmann's delusion was transmitted to Charles Martin on April 9. Martin had seen anomalous heat with heavy and ordinary water, it later transpired, due to a faulty connection that caused power to enter the cells unexpectedly. Having informed Pons of his "success" with heavy water, Martin then expressed concerns about the heat with ordinary water. Saying, "We see it too," Pons assured Martin that this was the most exciting result of his experiment, and he convinced Martin to go public. Thus on the April 10, Texas A&M made the first dramatic claims to reproduce the Fleischmann and Pons effect, amid much media excitement. Later, when they realized their error, they admitted it, but the media gave this retraction less publicity.

Within weeks, Fleischmann decided that the heat with ordinary water was not fusion and when asked once more—at the Electrochemical Society meeting in Los Angeles in May 1989—he dismissed it, even claiming that he saw no anomalous heat in ordinary water.

What are we to conclude from the cold-fusion saga? This depends on which of the versions you give the most weight to. Tapes of Fleischmann at CERN on March 31 leave no doubt that he

believed that no experiments had been done with ordinary water. Upon seeing heat with ordinary water, Pons believed it too, as long as it fitted with the pre-conceived nuclear hypothesis. Later realizing that this interpretation "didn't hold water," Pons found reasons within the collaboration to cause these particular results to be dismissed as unreliable.

There may be valid reasons for this conclusion that logbooks would enable historians to evaluate; however, based on other evidence, I suspect that this is a posteriori reasoning. The heat results with heavy and ordinary water indicate either calibration error or evidence of a chemical, but certainly not, nuclear process.

The Harwell experiments suggest that miscalibration may be the case.[5] I attach weight to these experiments because they were performed with Fleischmann's advice by a multidisciplinary team of experts aware of the many subtleties of the palladium/water system, and because a careful reading of the lab's paper gives clues as to the possible source of error in Fleischmann's and Pons' claims.[6]

The Harwell experiments have been reported widely as having "seen nothing." Harwell researchers initially set up cells based on the Fleischmann and Pons design and noticed that both with heavy and ordinary water there were possible heat imbalances. They commented that either there was a genuine effect or an error. They suspected the latter because at no time did the heat

production become statistically significant. They then repeated the experiments and saw no effect.

There is an opinion, propagated by true believers, that Harwell failed to see an effect because they used too low an electric current density. However, tables in the Harwell paper show that their currents often exceeded those with which Fleischmann and Pons claimed to have seen the effect.

The Harwell experiments ultimately showed thermal balance. Thus the insignificant heat in Fleischmann's and Pons' cells suggests there was some error in the calibration.

The evidence Fleischmann and Pons presented in their original publication[7] for nuclear radiation to support a nuclear process has been largely discredited.[8] Fleischmann himself described it as "rubbish" to The New York Times in 1991. Although several groups claimed to have found neutrons, tritium, or helium, there is no consistent pattern and no products commensurate with the heat claims. With the exception of a single claim to have found helium (and many experts dismiss this finding), no one comes within many orders of magnitude of what would be needed.

True believers invoke novel theories to explain the production of excess heat. It is unfortunate that groups, who insist that they produced heat, are so defensive about analyzing the cell for tell-tale products; or, having announced that assays are being done, fail to publicize the results or explain why there may be some fault in the analysis.

Absence of significant nuclear products in the Fleischmann and Pons experiment is now well accepted. Circumstances at the time they claimed to have found such evidence are disturbing because they raise questions about the conduct and responsibility of scientists in data presentation.

Conclusion

It is now possible to draw some conclusions on the status of the March 1989 experiments.

First, there is no evidence for neutrons (or, indeed, any nuclear remnants) produced in quantity commensurate with the heat. To most nuclear physicists and many other scientists, this implies that there is no nuclear fusion. To those who wish to believe that nuclear fusion takes place in test tubes of water, this is miraculously turned into evidence for "aneutronic fusion." However, in my opinion, the nuclear hypothesis is false.

Heat with both light and heavy water, together with absence of nuclear products commensurate with the heat, are the standard signals of a chemical process. Bursts of heat, which appear to be too great to be explained away as measurement error, may be due to an energy storage effect, though of a chemical nature. If so, I encourage those who research storage batteries to pursue this issue.

■ ■ ■ ■

NOTES:

1. For a more detailed discussion, see Frank Close, *Too Hot to Handle--The Race for Cold Fusion* (Princeton, NJ: Princeton University Press, 1991).

2. D.E. Williams, et al., "Upperbounds on Cold Fusion in Electrolytic Cells," *Nature* 342 (Nov. 23, 1989), p. 375.

3. M. Fleischmann, S. Pons, M. Anderson, L.J. Li and M. Hawkins, "Calorimetry of the palladium-deuterium-heavy water system," *Journal of Electroanalystical Chem*. vol. 287, no. 2 (1990), pp. 293-348.

4. Frank Close, *Too Hot To Handle*.

5. *Ibid*.

6. M. Flieschmann, S. Pons, (and M. Hawkins), "Electrochemically induced nuclear fusion of deuterium," *Journal of Electroanalytical Chem*. vol. 261, no. 2A (1989), pp. 187-188 and "Erratum," *Journal of Electroanalytical Chem*. vol. 263, no. 1 (1989), pp. 187-188.

7. *Ibid*.

8. W.J. Broad "Cold Fusion Claims Cited on Ethics as well as Science," *The New York Times*, March 17, 1991, p.1.

Sidebar with Cathy

Date: Mon, 24 Feb 1992 17:35 CDT
From: <DROEGE@FNALD>
Subject: I think it is over for MKF
To: rlg2@yktvmv
Original_To: JNET%"rlg2@yktvmv"
Original_cc: DROEGE

Dear Dick,

Will send you a copy of what I posted on MKF. Oriani has made similar measurements. Someone had called him and told him that I was getting a positive result. But our discussion really convinced me. He had done one more thing that I had not in collecting the gas and weighing the condensate.

As with all experiments that push the state of the art, the result may be interesting. What is happening? The easy explanations i.e. recombination seem to be unknown.

I will probably not publish anything formal. I would rather use my time to explore some other leads.

Mills sent his associate to the Bochris lab where he produced the claimed result. It remains to be seen whether Bochris believes that there is really excess heat. I takled with Bochris but he was in a hurry to go to class and I could not figure out whether he was a "believer" or not.

My position is that there is nothing that I have seen in the MKF experiment which leads me to believe that anything other than chemistry is involved.

Tom

==========

Date: Mon, 24 Feb 1992 17:37 CDT
From: <DROEGE@FNALD>
Subject: The fusion posting. See my cat in the 2 March Business Week
To: rlg2@yktvmv
Original_To: JNET%"rlg2@yktvmv"

From: FNALD::DROEGE 24-FEB-1992 14:44:58.31
To: GOV%"fusion%zorch@ames.arc.nasa.gov"
CC: DROEGE
Subj: Looks like it's over

We have continued to run MKF

1) Cell in calorimeter vented to atomsphere. Using powere correction of 1.48*I to correct for the vented gas the experiment is exothermic.

2) Measure gas flow from 1). Looks like 100% efficient electorlysis. +/- 5%.

3) Add external 6 cm mercury bubbler and catalyst, and measure any excess gas coming off reaction. Balance as in 1) maintains excess heat. i.e. the presence of the catalyst does not kill reaction. Some excess gas seen.

4) Move bubbler and catalyst into calorimeter, maintaining same tubing length between cell and bubbler and bubbler and catalyst. Energy balance goes endothermic, moving to near zero after time. Now 1.48*I correction does not apply.

5) Move bubbler and catalyst back out of calorimeter. Again hold same tubing length and configuration. Also not apply 1.48*I correction. Now the reaction is again exothermic.

6) Running as in 5) some time notice that even though the electrolyte is becomming more concentrated as water is lost from electrolysis, that the cell voltage is going up. Possibly indicating the loss of something.

That should be "Also now apply 1.48*I correction." in 6)

The advantage of paying my own way is that I don't have to keep working because I have a grant. So I will likely vote with my feet and go on to something else. But it was fun while it lasted. I recommend the practice. Get jobs as ditch diggers or computer programmers or some other honest work and finance your own science. Those fancy machines you are all addicted to can always be had if you have a good idea.

The correction is to 5) not 6) Some day I will learn to use the editor.

Tom Droege

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July 8, 1991
(Via FAX to 9 (202) 371-9227)

Ms. Katherine Livingston
Book Review Editor
Science
1333 H Street, NW
Washington, DC 20005

Dear Ms. Livingston:

Thank you for your letter of 07/01/91 inviting me to review for Science the Mallove book "Fire From Ice: Searching for the Truth Behind Cold Fusion."

I would be glad to provide this review by end-August, but I would need to know the precise length that would be acceptable.

Furthermore, once I provide the review, I am perfectly willing to have the text marked up by you or some Editor at Science, but not retyped, and I would make those modifications that seemed appropriate to me, in view of the markings.

I would not undertake this work without a definite understanding that the final choice of words would be mine, and that Science would only mark and not retype my manuscript. I would be glad to provide the manuscript triple spaced, if that is preferred.

Sincerely yours,

Richard L. Garwin

Richard L. Garwin
Forwarded in his absence

RLG:jtml:Q189KL:070891..KL

5he   [illegible]   ← Srinivasan[?]

R Ch   115-118, 127, 163,
166, 187, 306

↑ Bazzar[?]   ↑ RPAB[?]   ↑ Ch[?] [illegible]

10^4 dpm/ → 10^2 dps × 6×10^8

→ $6\times10^{10}/10^6$

6×10^4

$6\times10^{-6}\times10^{-7} = 6\times10^{-13}$

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09 July 1991

Richard L. Garwin
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I am glad that you are willing to take on the review of Mallove's FIRE FROM ICE, and the book is being sent to you today, under separate cover.

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Yours sincerely

Katherine Livingston

Katherine Livingston
Book Review Editor

KL: bpr

5030 ✓

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about 70 W/cm heater tape power

ELEVATED-TEMPERATURE EXCESS HEAT PRODUCTION IN A Pd-D SYSTEM

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INTRODUCTION

The production of excess heat in a Pd-D system at ambient temperatures, as reported by Fleischmann et al. [1], suggested the possible emergence of a nearly inexhaustible, clean, inexpensive source of energy. Although many negative results have been reported [2], increasing numbers of positive results have also been reported [3,4] in the detection of excess heat, tritium, and neutrons, either separately or collectively, despite the fact that the nature of this reaction has not yet been conclusively identified.

For practical purposes, the use of aqueous electrolytes in most of these studies has limited the employment of this technology to relatively low temperatures, typically below 100°C, unless a pressurized cell is used. The use of an elevated temperature approach, however, would hold an obvious advantage: in terms of thermodynamics and kinetics, the efficiency of electricity conversion and the diffusion of deuterium in palladium can both be increased.

It has been suggested [1] that high deuterium activity in palladium deuteride is critical to achieving excess power and heat. It is also known from the Pd-H phase diagram [5] that, at the same H/Pd ratio, the hydrogen partial pressure, as well as the deuterium activity in metal deuterides, increase quite significantly with temperature.

The use of molten-salt electrochemical techniques to generate excess power and heat at elevated temperatures, typically above 350°C, is presented here. The use of molten-salt techniques to study metal-hydrogen systems was recently illustrated by Luedecke et al. [6,7] and Liaw et al. [8,9], while the underlying principles were explicitly described by Deublein and Huggins [10]. The molten-salt electrolyte in this study is a eutectic LiCl-KCl mixture saturated with LiD, which has a melting temperature of about 350°C. LiD provides the deuteride-conducting species, D^- ions, in the electrolyte, along with a very strong reducing environment that removes surface oxides on metals and thus facilitates deuterium reaction with the metal. Many transition metals and their alloys [11,12], which can absorb substantial amounts of deuterium, often suffer from an impeding oxide surface layer in aqueous systems. Therefore, these transition metals can be utilized in this molten-salt environment. Since quite a considerable amount of LiD can be incorporated into the melt, the electrolyte has high ionic conductivity. Accordingly, we

believe that this approach is superior to the aqueous system for producing excess heat in metals for many reasons, including operation at elevated temperatures to ensure higher thermodynamic efficiency, use of less expensive materials, and, possibly, a higher gain in power through better kinetics.

PRINCIPLES

Recently, Deublein and Huggins [10] proposed a novel approach using a eutectic LiCl-KCl molten-salt, saturated with LiH, as an effective means of preparing hydrogen-transparent metal surfaces. The same technique, demonstrated by Liaw et al. [8,9] and Deublein et al. [13-15], showed that a strongly reducing hydride-conducting electrolyte could be used to control metal-hydrogen reactions in electrochemical cells. The alkali hydride used in the electrolyte dissociates in the melt and forms alkali and hydride ions. The hydride ion can thus be readily transported in the electrolyte and metal hydrides are produced electrochemically.

We expect the deuterium system to behave in a similar fashion. Therefore, an electrochemically-induced reaction, similar in nature to that reported by Fleischmann et al., can be conducted using a cell, as shown in Figure 1. The cell half reactions are:

At the anode: $$Pd + x\,D^- = PdD_x + e^- \quad \text{(i)}$$

At the cathode: $$Al + Li^+ + e^- = \text{"LiAl"} \quad \text{(ii)}$$

which give the total cell reaction:

$$Pd + x\,Al + x\,LiD = PdD_x + x\,\text{"LiAl"} \quad \text{(iii)}$$

with an endothermic reaction enthalpy, ΔH_r, estimated at approximately 3 kJ mol^{-1} at 700 K [16].

The dynamic heat balance of the cell was periodically monitored by a thermocouple in an isoperibolic calorimeter. The heat balance during electrolysis resulted in a change in the emf of the thermocouple and the resulting output power was subsequently determined from calibration data on the correspondence between power and the equilibrium thermocouple response.

EXPERIMENTAL ASPECTS

The cell shown in Figure 1 consists of an Al container, Al crucible, and dewar flask densely packed with glass fiber (Kaowool, 8# density) insulation. Two Pd samples (Engelhard, 4 mm dia. wire, 99.99%) were torch-melted with a propane/oxygen flame. One of them, weighed 0.4874 g before use, was wound with a molybdenum wire (from Alfa Products, 0.127 mm dia.) to a steel current collector and used as the positive electrode (anode). The other was later used as a blank. A section of Al tubing (700/SF 6061), 2.54 cm long and 0.32 cm thick, was used as the negative electrode (cathode) to react with Li. The Al electrode was continuously replaced after being fully loaded with Li. Current collectors were steel rods, 0.3 cm in diameter. A high-temperature, glass-fiber insulated heating tape surrounding the near bottom of the container was the heat source for maintaining cell temperature above 350°C. The power to the heating tape was supplied by a Novatron DCR 80-10 DC power supply (max. ±80 V, 10 A), manufactured by Sorensen/Raytheon Co. of South Nowark, Connecticut. The power could be easily maintained at a level of 50-80 W with a variation within one tenth of a watt. It should be noted that the power to the heating tape was kept constant throughout each experiment and its rest temperature was recorded before calorimetric work was performed.

Eutectic LiCl-KCl (47.6 wt% KCl + 51.9 wt% LiCl) was prepared by the Lithium Corporation of America and used as received. The eutectic melt (about 20 cm^3) was heated inside the crucible for at least one day before 3-5 g LiD (Aldrich, 98+ at% D) was added. This process helped to reduce the initial consumption of LiD by the moisture contaminant in the melt. The experiments were carried out in a controlled argon-filled glove box in which oxygen and moisture were continuously removed.

The calorimetric measurements were based on a dynamic heat balance determined by a K-type (chromel-alumel couple from Omega Engineering, Inc.) thermocouple in the electrolyte. The thermocouple was shielded with a ungrounded stainless steel sheath 0.1 cm in diameter. Because there was a large mass of metallic parts in the system, their conductive properties aided in the thermal equilibration of the dynamic heat balance. The ionic melt also conducts heat effectively. Based on this preliminary study, we believe that the local heating problem was considerably reduced with

this approach.

The calibration procedure was conducted by employing the resistor heating tape, which was in contact with the reaction cell container, as a joule heat source. By varying the power to the resistor heating tape, P_r, we obtained a corresponding ΔT versus P_r relation as a calibration curve. The time constant of the calorimeter was in the range of 5-6 hours, which was the time corresponding to imposing a power step on the system and the subsequent thermal equilibrium. An interval of at least 12 hours separated each calibration step. It should be noted that all the temperature values reported in this paper correspond to the temperature difference between the cell temperature represented by the thermocouple reading and the ambient temperature of the glove box, which was maintained between 25-30°C and constantly monitored by a thermometer inside the box. The calibration curve obtained after the high-current excursion was used in the interpretation of calorimetric data.

Two separate calorimetric experiments based on the Pd-D system were conducted. The Pd sample was weighed 0.4874 g before use and had an irregular shape due to melting. The surface area of the Pd was estimated to be about 0.99 cm^2. It was charged for more than three weeks at 4 mA cm^{-2} to ensure complete loading of deuterium throughout the sample before later use for high-current excursions. After this pre-charging period, LiD was replenished and another short period of charging at low current density was performed before switching to high current density charging. This ensured a high loading state for each experiment. All electrochemical work was performed with an EG&G Princeton Applied Research PAR-173 potentiostat/galvanostat under galvanostatic operation.

Scanning Electron Microscopy or SEM (ISI Model SX-40A with a Princeton Gamma Technology Energy-Dispersive X-ray or EDX Elemental Analysis System) was used to examine the morphology of the Pd samples. The different morphology between the deuterated sample and the blank will be described.

RESULTS

Figure 2 shows the time-dependent temperature excursion of the PdD_x | LiD, eutectic LiCl-KCl | Al cell subjected to three high current density charging experiments. Before the high current density charging, the Pd was charged at 2.5 mA cm^{-2} after a new

charge of LiD was added to the melt in order to ensure high deuterium loading. After 88 hours of charging at the low current density, the current density was raised to 290 mA cm^{-2} and a subsequent temperature rise was recorded. It took more than 6 hours to reach a relatively steady temperature of 407°C, a 30°C increase from an electrochemical power input of only about 0.36 W. About 25 hours later, the current density was increased to 420 mA cm^{-2}, which corresponds to an input electrochemical power of 0.92 W, and the temperature increased to 419°C. Another 47 hours later, the current density was 692 mA cm^{-2}, for an input electrochemical power of 1.68 W, and a resulting temperature of 460°C. A larger fluctuation in temperature was observed at this stage, which lasted for another 26.8 hours because, presumably, the LiD was exhausted. The cell returned to its initial rest temperature about 6 hours after the termination of the input electrochemical power. This demonstrated that the thermocouple was functioning normally; no degradation of the thermocouple was observed.

Figure 3 shows the results from the above experiments and another one charged at 606 mA cm^{-2}. Two calibration curves and the corresponding temperature response of the PdD_x | LiD, eutectic LiCl-KCl | Al cell under four different charging current densities are shown, with the values given in Table 1 for the measured cell voltage, current density, input heating power, input electrochemical power, output power, as well as the calculated excess power and heat. The total input power is the sum of the power supplied to the resistor heating tape and the electrochemical work applied to the cell; i.e., the product of cell potential and current ($E_c \cdot I$). Although the cell was an open system, the thermoneutral potential of the total cell reaction was not considered in the calculation of the input electrochemical power.

Based on the calibration data, we can convert the temperature reading into a measurable output power, as depicted in Figure 4, for the experiments shown in Figure 2. The difference between the measured output power and the total input power represents the excess power from the reaction. The area between the two curves is, therefore, the excess energy that was produced during the high-current-density charging experiments. Integration of the curves yields an excess energy of 5.02 MJ for this particular set of experiments, which was limited by the amount of LiD present in the cell. The electrochemical charge applied to the cell during the charging period was about 154,600 Coulombs, which corresponds to

0.801 moles of D_2 gas. Thus, an excess heat of 6.26 MJ mol^{-1} D_2, or 1096 MJ mol^{-1} Pd, was obtained. This number is comparable with the number derived from the steady state values, as shown in the last column of Table 1.

The magnitude of power and energy measured in these experiments was surprisingly high, as shown by the last entry in Table 1. The input power to the heating tape was maintained at about 69.25 W, the cell potential was typically in the range of 2.45 V, and the input electrochemical power was about 1.68 W at 692 mA cm^{-2}, which corresponds to a total input power of about 70.9 W. Based on the calibration curve, we would expect the 1.68 W of joule heating to result in a 5.1°C increase in temperature; however, the temperature increased by 82.4°C. In other words, this temperature rise corresponded to a power level of about 27.1 W, according to the calibration curve. Therefore, a net gain of 25.4 W was in excess, which resulted in an excess power gain of 1512%, in the range of 627 W cm^{-3} Pd.

The surface morphology of the Pd samples is shown in Figures 5a-5e. Figures 5a and 5b are of the blank Pd sample that did not experience electrolysis, while Figures 5c-5e are of the Pd sample that was deuterated during the high current excursion experiments. EDX results on the deuterated sample indicated some Fe and Zn contamination after electrolysis, as shown in Figure 6a. These contaminants were not detected by EDX in the blank (Figure 6b).

DISCUSSION

Thermochemical Aspects

Most of the thermodynamic data, especially the enthalpies, which represent heat associated with the formation of compounds and the solution of hydrogen in the system, can be found in the open literature [17]. When all reactions that are known to occur in the system are considered, no reason can be found that would justify attributing a thermochemical reaction to the excess power generation. Thus, these results suggest that the effect is non-chemical.

The reported enthalpies of the compounds related to the system are compiled in Table 2. Table 3 lists possible chemical reactions and their corresponding reaction enthalpies, as calculated from Table 2. All calculated enthalpies are positive for every possible reaction involving hydride or deuteride formation, hydrogen dissolution, gas

evolution, and chloride formation, indicating that these reactions are endothermic in nature and should not contribute to any excess power or heat measured.

Therefore, the origin of the excess heat generation can only be attributed to a nuclear process or, maybe, several processes, which are as yet unknown. There is increasing evidence [3,4] from other laboratories that tritium and neutrons have been detected in deuterated metals, which directly supports the hypothesis of a nuclear phenomena. Our recent He analysis [18] of the two samples showed that ^4He was significantly above the background in the deuterated Pd sample but not in the blank, which is further evidence of a nuclear phenomena. However, it seems that the phenomena are different and far more complicated than what have been learned from high-temperature high-energy conditions in physical confinement methods.

Experiments based on LiH have been performed and will be discussed elsewhere [19]; however, no excess heat in that system has been found to date, which suggests that there is indeed a difference between the deuterium and hydrogen systems. The calorimetry results from the Pd-H system also agree with what are expected from thermochemical reactions, verifying the correct response from the calorimetry techniques.

Morphological Aspects

The most intriguing features of the SEM studies is the large amount of porosity found in both as-prepared and electrolyzed samples. These porous features were introduced by torch melting, resulting in a grain and pore size typically in the range of 300-500 μm (Figure 5a) and 3-5 μm (Figure 5b), respectively. The deuterated sample shows a larger pore-size distribution in the range of 10-50 μm and sometimes up to 100s of μm. There is no evidence of any grain structure in the deuterated sample.

It is known that Pd is quite ductile but becomes brittle after hydriding. We often found that portions of highly deuterated or hydrated samples were easily broken into pieces. The waffle-like surface morphology of the non-deuterated sample (Figure 5b) may suggest that the formation of the pores resulted from the following process: a considerable amount of gas (primarily CO_2 and H_2O) were introduced and forced into the molten Pd by the torch flame. The gas

formed highly dispersed bubbles in the molten Pd. Upon solidification, the gas bubbles near the surface were forced out and escaped at the surface, leaving porous channels and rough flake-like thin walls on the Pd surface.

The dramatic morphology change by electrolysis is evidenced by the comparison of Figures 5c-5e with 5a and 5b. The much rougher surface shown in Figure 5c implies that a new layer has been formed during electrolysis with evidence of dendritic growth (Figure 5d). This new surface is predominantly Pd, according to EDX results (Figure 6a). We suspect that high current charging excursions resulted in this process in parallel with the deuterium transport. Quite possibly, the electrochemical deuterizing process had a parallel process that caused substantial Pd dissolution (possibly by means of chloriding), electromigration and diffusion, and electroplating of a new Pd layer, which resulted in the microstructural rearrangement. In some locations, a needle-like crystalline phase was found and was identified by EDX to be pure Pd, as shown in Figure 5e. The transport behavior of deuteride ion in the chloride melt is still under investigation.

The presence of Fe and Zn cannot be quantified from the EDX results (Figure 6a). The Fe contaminant possibly came from the steel current collectors. The Zn contaminant was possibly from the Al electrode (since 6061 Al alloys contain 0.25% of Zn). Whether these contaminants promote excess heat production is unknown; however, it is unlikely that either their presence and associated chemical reactions or the Pd dissolution/plating process would be the source of excess heat, simply because the amount of these substances involved is far too small to be accounted for by the energy that was measured.

The significance of the morphology change of the Pd electrode in association with the excess heat production is not clear either. But the interesting features that we discovered in this experiment, along with our failure to reproduce excess heat using untreated Pd wires with the presence of macrocracks and microcracks, suggest that the interplay of microstructures and deuterium insertion into Pd may be of paramount importance.

CONCLUSIONS

We have demonstrated that, employing novel molten-salt electrochemical techniques, excess power production was found in the Pd-D system. In a particular case, a large amount of excess heat of 6.26 MJ mol^{-1} D_2 was measured in a Pd-D charging experiment. The morphology of the Pd electrode may have a significant impact on excess heat production. The production of excess heat and the control of morphology of the Pd electrode have been difficult to replicate.

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16. Due to lack of the formation enthalpy data of the palladium deuteride at elevated temperatures, the reaction enthalpy was estimated from that at 298 K in Table 3.

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Figure Captions:

Figure 1. Schematic drawing of the elevated-temperature molten-salt electrochemical cell.

Figure 2. Temperature variation during high-current-density charging experiments in the Pd-D system. Numbers are the charging current densities in mA cm^{-2}.

Figure 3. Power balances during calibrations and high-current-density charging experiments. The calibration curves were obtained after each charging experiments and indicate the total input power-temperature relationships of the cell. The data of the charging experiments show that the temperature increases due to the total input power exceed what would be expected from the calibration curves respectively. The charging current densities were: a, 606; b, 290; c, 420; and d, 692 mA cm^{-2}.

Figure 4. Comparison of the input and output power of the Pd-D system during the high-current-density charging experiments. Numbers are the charging current densities in mA cm^{-2}.

Figure 5. a) Low magnification SEM topography of a blank Pd sample that has been torch-melted. Grain features are readily recognized as well as the high density of pores. b) High magnification view of the surface pores structure on the blank Pd sample. c) Change in surface morphology after high-current-density deuterium charging experiments with no evidence of pre-existing grain structure. d) and e) Dendritic growth of Pd is shown, accompanied by needle-like crystalline Pd precipitates. The white marker at the right lower corner on each figure represents a scale for a) 40 μm, b) 2 μm, c) 40 μm, d) 2 μm, e) 2 μm, respectively.

Figure 6. EDX elemental analysis of a) the Pd sample after the high-current-density deuterium charging experiments (averaged over the area shown in Fig. 5c) and b) the blank. In Figure 6a, Fe and Zn were identified as minute contaminants possibly arising from the cell components.

TABLE 3

Calculated enthalpies of reaction in the Pd-D system at 298 and 700 K

Reaction	ΔH_r at 298 K /kJ mol^{-1}	ΔH_r at 700 K /kJ mol^{-1}
$0.56\ LiH + Pd + 0.56\ Al = 0.56\ LiAl + PdH_{0.56}$ (hydride formation)	12.13	
$0.56\ LiD + Pd + 0.56\ Al = 0.56\ LiAl + PdD_{0.56}$ (deuteride formation)	13.68	
$\delta\ LiH + PdH_x + \delta\ Al = \delta\ LiAl + PdH_{x+\delta}$ (hydrogen dissolution)	23.05	2.13 (x=0.35) 29.74 (x=0.65)
$LiH + Al = LiAl + 1/2\ H_2$	41.66	41.45
$LiD + Al = LiAl + 1/2\ D_2$	41.95	
$2\ LiCl + PdH + 2\ Al = 2\ LiAl + PdCl_2 + 1/2\ H_2$	556.55	
$2\ LiCl + PdD + 2\ Al = 2\ LiAl + PdCl_2 + 1/2\ D_2$	555.17	

TABLE 2

Enthalpies of compounds involved in the Pd-D systems at 298 and 700 K [1]

Compounds	ΔH at 298 K /kJ mol^{-1}	ΔH at 700 K /kJ mol^{-1}
LiH	-90.60	-94.695
LiD	-90.90 [2]	
LiCl	-408.169	-408.491
KCl	-436.58	-436.54
$PdCl_2$	-173.13	-166.47
LiAl	-48.94	-53.237
PdH_x		
(β hydride formation)	-11.21 (x=0.56, 303 K) [3]	
dissolution	-37.23 (in metal, 303 K) [3]	-39.3 (x=0.35) [4] -11.7 (x=0.65) [4]
PdD_x		
(β deuteride formation)	-9.83 (x=0.56, 303 K) [3]	

(1) Data are from I. Barin, O. Knacke, and O. Kubaschewski, "Thermochemical Properties of Inorganic Substances," Springer-Verlag, Berlin, 1973 and 1977.
(2) From "CRC Handbook of Chemistry and Physics," 65th edition, ed. by R. C. Weast and M. J. Astle, CRC Press, Florida, 1984-85.
(3) Data are from G. G. Libowitz "The Solid State Chemistry of Binary Metal Hydrides," Benjamin, N. Y. 1965.
(4) ΔH decreases in magnitude with increasing H/Pd. Data are from P. L. Levine and K. E. Weale, J. Chem. Soc. Faraday Trans. 56, 357 (1960).

TABLE 1

Parameters and power balances in the Pd-D experiments

Cell voltage, /V	Current density /mA cm^{-2}	Power to heating tape, P_r /W	Electro-chemical power /W	Total input power /W	Power output measured /W	Excess power /W	Excess power gain /%	Excess heat /MJ mol^{-1} D_2
3.230	606	71.91	1.94	73.85	86.76	12.91	665	- 4.15
2.188	290	69.25	0.63	69.88	79.24	9.36	1486	- 6.27
2.270	420	69.30	0.94	70.24	82.81	12.57	1337	- 5.83
2.453	692	69.25	1.68	70.93	96.34	25.41	1512	- 7.16

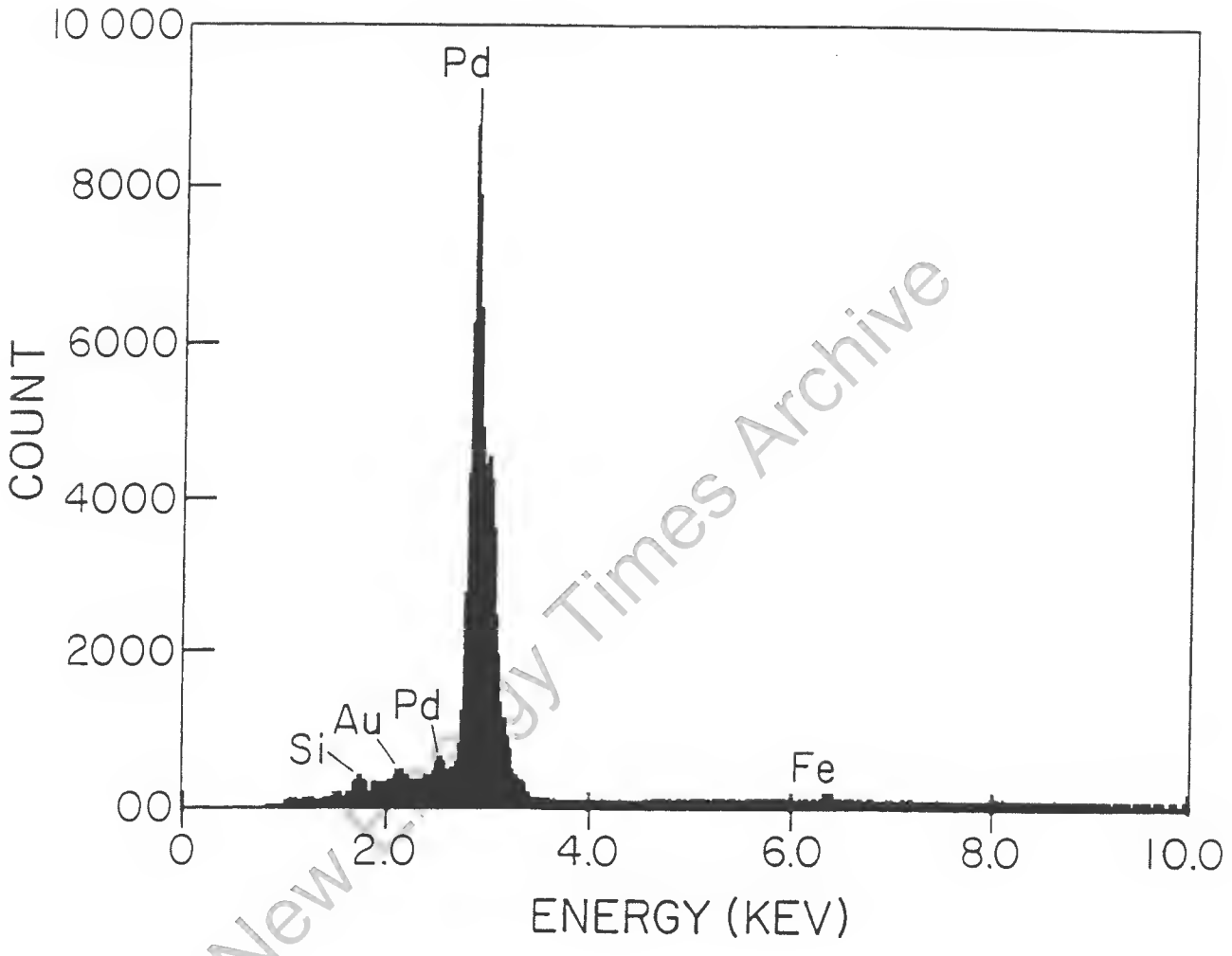

Figure 6b
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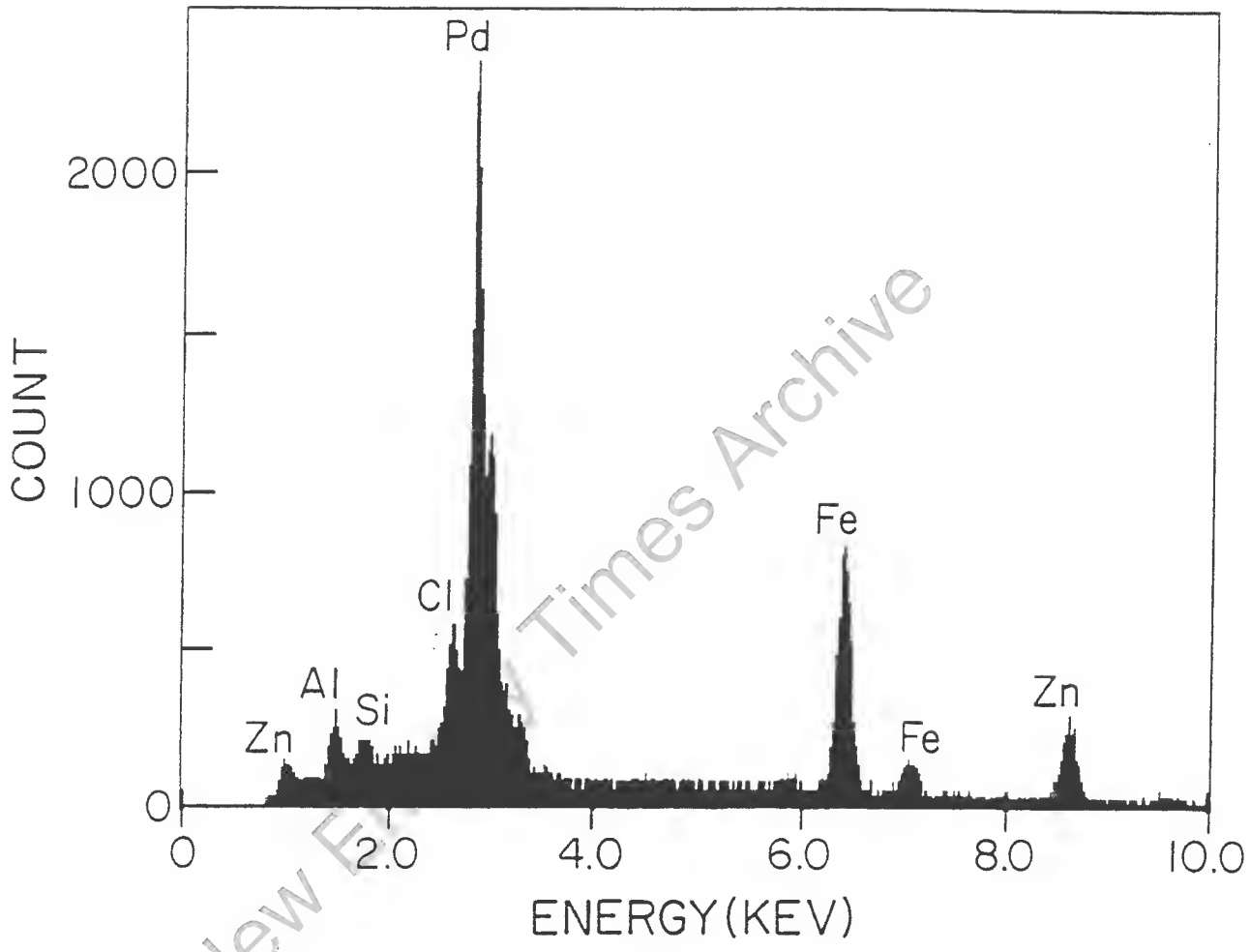


Figure 6a
Liaw, Tao, Turner, Liebert

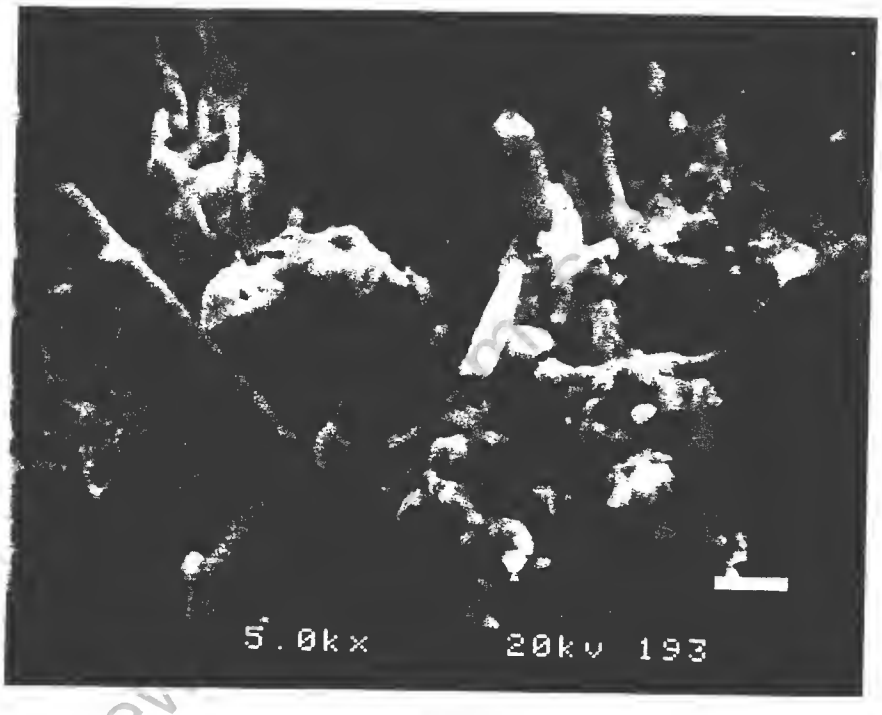
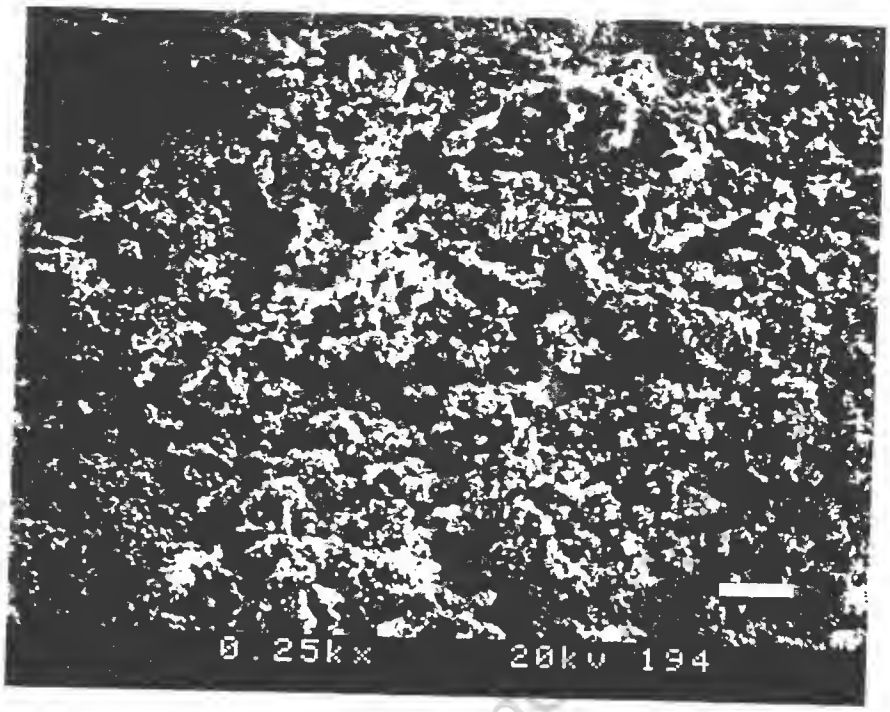


Figure 5e
Liaw, Tao, Turner, Liebert



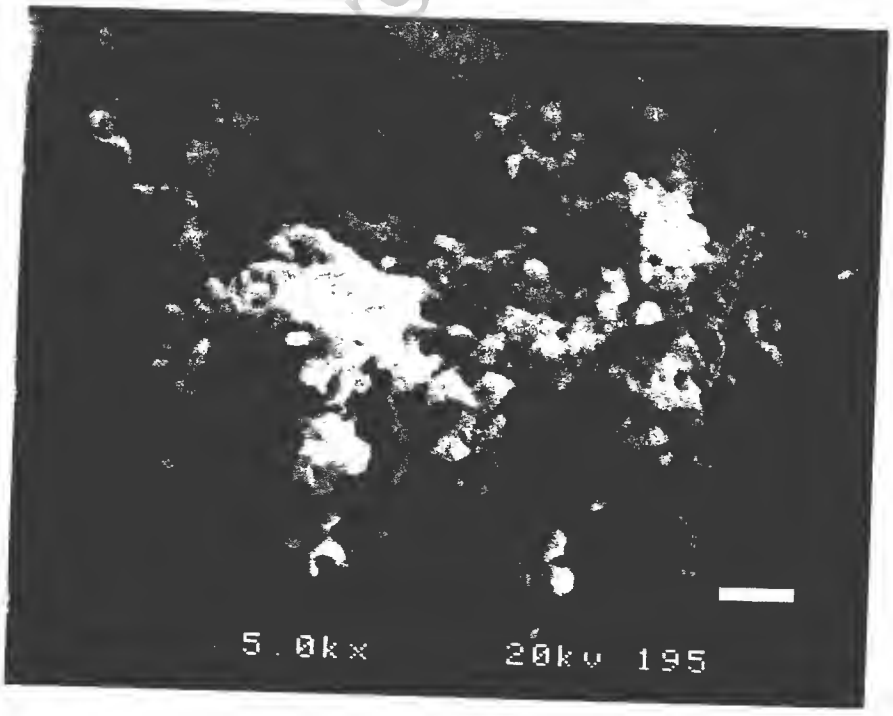


Figure 5c and 5d
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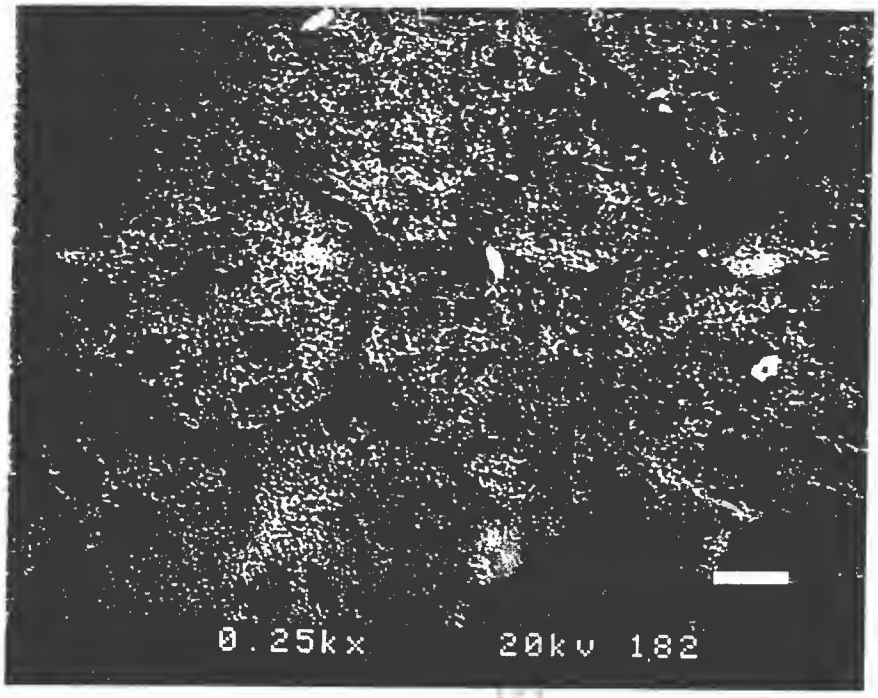

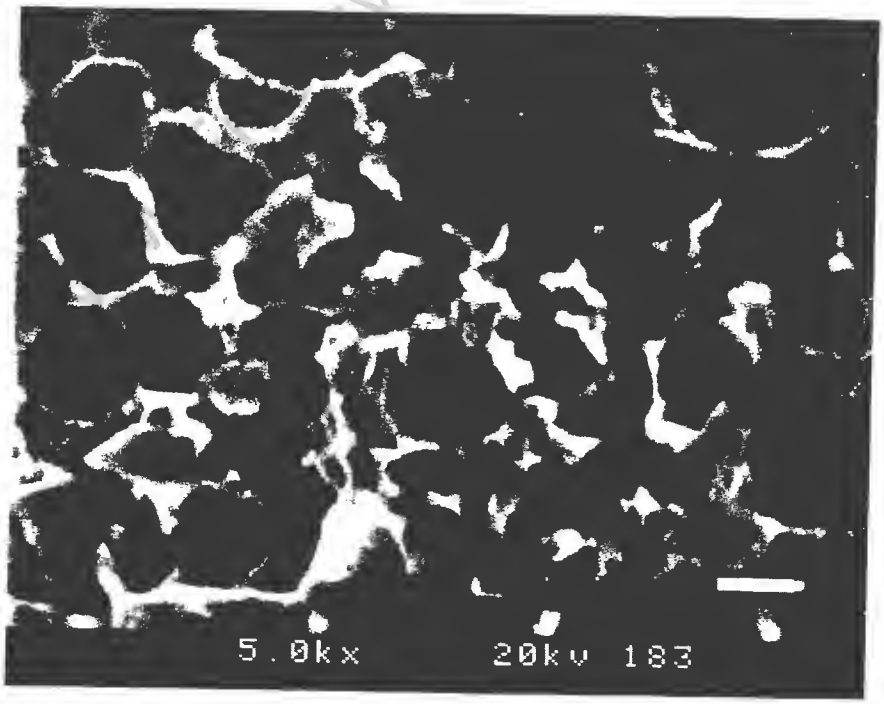

Figure 5a and 5b
Liaw, Tao, Turner, Liebert

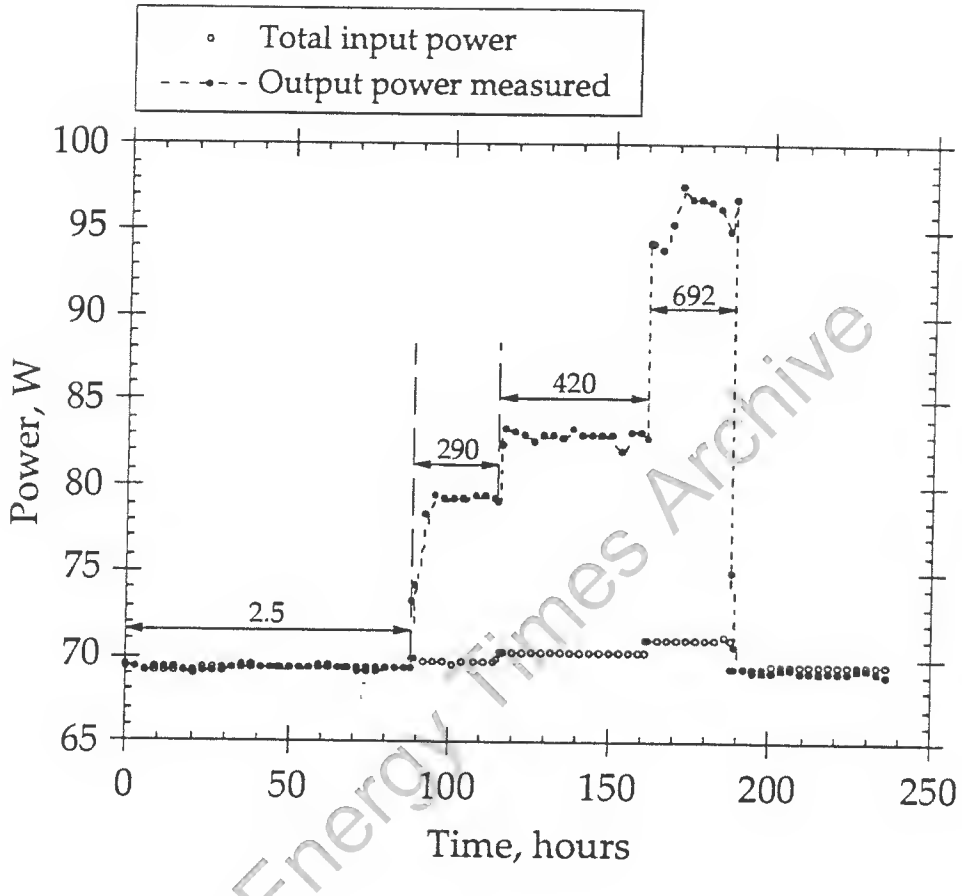

Figure 4
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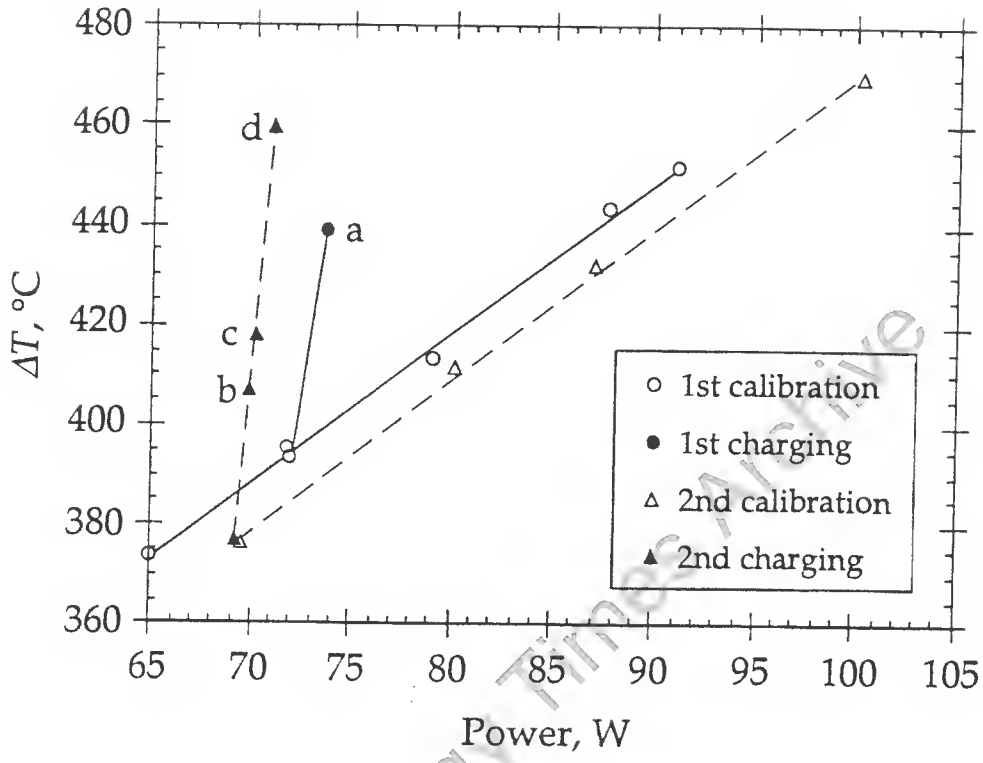

Figure 3
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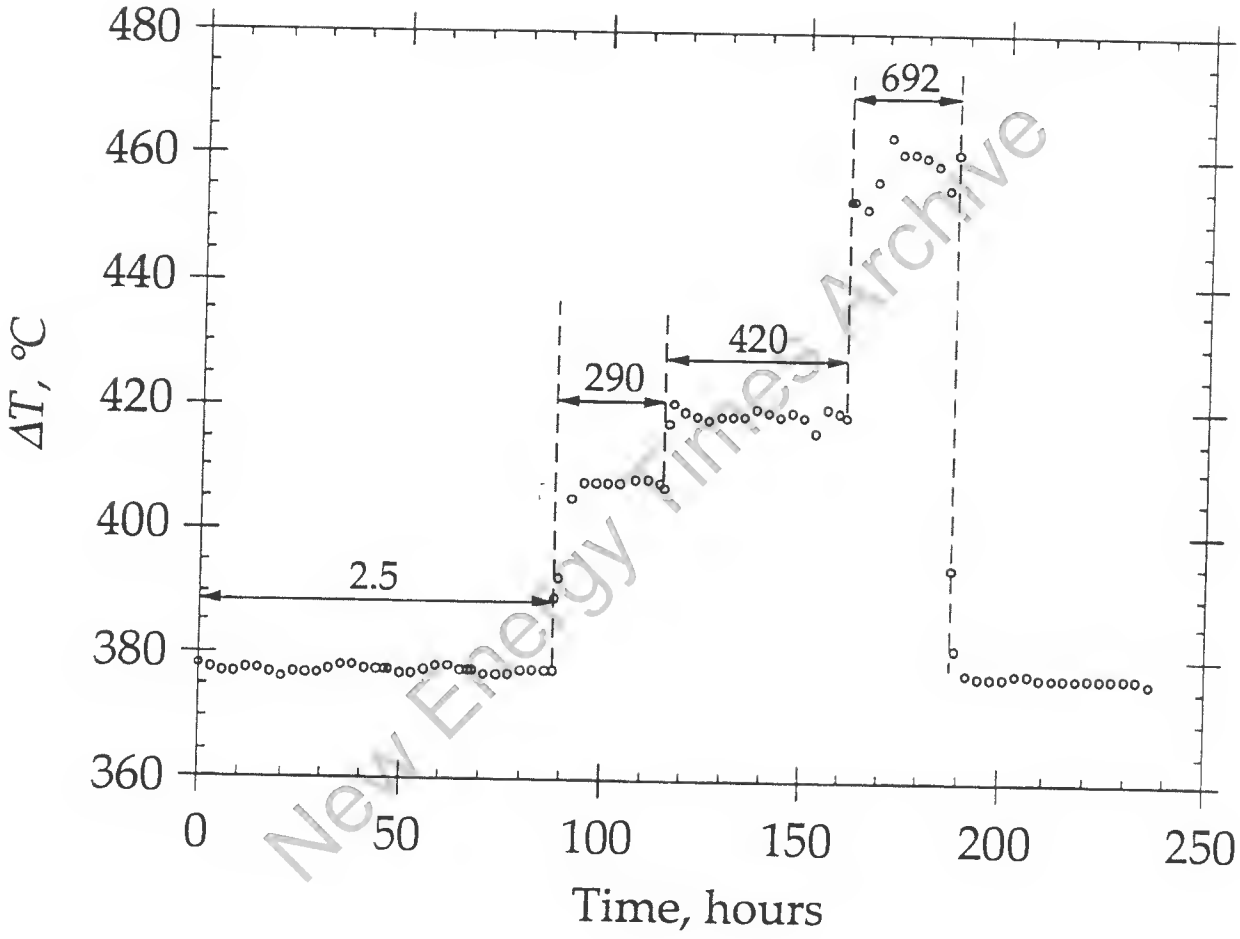

Figure 2
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ROUGH DRAFT

Darwin

LOW-BACKGROUND MEASUREMENTS OF NEUTRON EMISSION FROM Ti METAL IN PRESSURIZED DEUTERIUM GAS

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ABSTRACT

A wide variety of neutron detector systems have been used to search for anomalous neutron emission from deuterated metals. Some of the detector systems that have been used are summarized together with possible sources of spurious signals from electronic noise. During the past two years, we have performed experiments to measure neutron emission from pressurized D_2 gas mixed with various forms of titanium metal chips and sponge. Details concerning the neutron detectors, experimental procedures, and results have been reported previously. Our recent experiments have focused on determining a trigger mechanism for the anomalous low-level neutron emission. To improve our detection sensitivity, we have increased the shielding in our counting laboratory and we have located additional detector systems in deep underground counting stations. An update on this experimental work is presented.

INTRODUCTION

During the past two years, a considerable amount of work has taken place in an attempt to detect neutron emission from deuterided metal systems. The proposed nuclear reaction is

$$d + d \rightarrow {}^3He + n\ (2.45\ MeV)\ .$$

This reaction competes with the fusion reactions yielding tritium (T) and protons (P) or ^4He. In general, neutrons have the desirable property that they readily penetrate the sample, container, and the experimental apparatus.

High efficiency neutron detectors have been designed to measure the neutron production rate in bulk samples that include the entire experimental sample. For low background underground experiments, the sensitivities can be good enough to detect a few d,d fusion events per hour from the samples.[1]

In general, neutron detection is the most sensitive method to measure the possible fusion reactions. However, the absences of neutron signals has lead to the proposed[2] anomalous branching ratios of $n/T \sim 10^{-8}$ (tritium experiments) and $n/^4He < 10^{-12}$ (heat experiments). Additional experiments are required to establish if these anomalous ratios are true or if they represent the sensitivity limits of the experiments to measure neutrons or tritium or heat.

For neutron detection, the detector parameters of interest include the efficiency, neutron energy resolution, pulse time information, sensitivity to gamma-ray and cosmic-ray backgrounds, and noise susceptibility. These characteristics, together with shielding, will determine the sensitivity of the system to measure low-level neutron signals.

This paper describes some typical neutron detectors that have been used for cold fusion type experiments, and it gives some of the sources of false signals and possible techniques to protect against the artifacts. Also included is an update on the recent neutron measurements that we have performed at Los Alamos.

NEUTRON DETECTORS

A wide variety of neutron detectors have been used for the investigation of neutron emission from deuterated metallic lattice experiments. A summary of the publications corresponding to these experiments can be found elsewhere.[3,4] Table I gives a listing of the detector types that have been used in the experiments. The total neutron detection efficiencies range from 10^{-5} to 0.44 with the ^3He systems generally giving the higher efficiencies. Note that for neutron coincidence or time-correlation counting, two or more neutrons from a single event must be counted and the coincidence counting efficiency varies as the square of the singles efficiency. Thus, low efficiency detectors are not well suited to measure neutron coincidence burst events.

There is a basic difference between the thermal-neutron detectors and the fast-neutron detectors listed in Table I. The advantages of the thermal-neutron detectors include

1. higher efficiency,
2. simpler operation,
3. no gamma-ray sensitivity, and
4. burst detection capability by moderator thermalization time.

The advantages of the fast-neutron detectors include

1. neutron energy spectra,
2. fast time information, and
3. lower backgrounds.

The experiments that use a combination of detector types are especially good for noise rejection because the detectors are usually vulnerable to different types of problems.

Some possible sources of noise giving false neutron signals are listed in Table II. Most of the noise events are electrical in nature but some are nuclear in that cosmic-ray events and radioactive decay might be misinterpreted.

Table III lists some of the techniques that can be used to protect an experiment from noise events and/or to flag noise event in the data analysis. Some of these safety measures are mutually exclusive such as electromagnetic interference (EMI) shielded amplifiers at the detector head vs analog signal analysis in the electronics cabinet. In general, experiments that have been performed during the past year have incorporated more of the noise protection techniques than the experiments during the prior year. As long as the neutron emission results remain intermittent and irreproducible, a great deal of attention must be paid to the noise vulnerability question.

To gain better sensitivity in the experiments, it is necessary to reduce the cosmic-ray background signal by electronic means or shielding or both. The true neutron background has a random component from the decay of radioactive elements and a time-correlated component from cosmic-ray spallation reactions in the sample or detector body. The time-correlated background can be greatly reduced by performing the experiment underground. For example, the coincidence background decreases by a factor of 10^3 in the 70-m-deep tunnel location at Los Alamos and by a factor of 10^5 at the deep-mine locations at Leadville, Colorado, and Kamioka, Japan (1000 m). The coincidence neutron background rate is only ~0.1 counts/d for a 32% efficient ^{3}He detector in the Leadville tunnel location.

HIGH-VOLTAGE LEAKAGE NOISE TESTS

In response to an observation[5] that electronic noise bursts can be caused by moisture condensation in the high-voltage (hv) section of the detector during liquid nitrogen (LN) temperature cycles, we performed a series of experiments to look for this problem. A low-temperature cycle of the sample in the detector can reduce the detector temperature so that moisture condensation might cause high-voltage leakage on the signal line. This problem normally is prevented by the presence of desiccant in the hermetically sealed high-voltage box. However, if there is an air leak into the box, the interior condensation can occur under humid air conditions.

For the tests, we directly applied steel pieces (~2 kg) that had been cooled by liquid nitrogen LN to the hv junction box and the detector body. The counts from the detector were collected for 8 to 12 hours as the system returned to room temperature. The cooling-warmup cycle was repeated about 10 times for the three ^{3}He detector systems 1, 3, and 4 listed in Table IV.[6] Detectors 1 and 4 demonstrated no vulnerability to the noise tests. However, detector 3 gave intermittent noise bursts during the warm-up period, but the noise occurred only on humid days (rainy days). To enhance the problem, we placed detector system 3 in a plastic bag containing water to increase the relative humidity to ~100%.

All of the noise events had a time-correlation count greater than 50. The cosmic-ray background rate for coincidence counts is 1-2 counts/h and no excess of small correlation events above background was observed in any of the detector systems during the tests.

Our conclusions from these tests are that the ^{3}He systems can be vulnerable to hv noise from moisture condensation under temperature cycling in humid conditions. Detector systems that have effective air seals and drying agents such as desiccant are not subject to the problem.

Our newer detector configurations have the signal lines segmented to give independent readouts of ^{3}He detector banks and the ratio of the segments easily identifies hv leakage noise events. However, some of our prior results[6] using detector system 3 were subject to this noise problem.

SAMPLE CHARACTERISTICS

In October 1990, we reported[7] neutron emission results using a consistent sample preparation procedure involving clean samples and high purity gas preparation. Under these conditions, the titanium (Ti) samples would readily absorb deuterium gas after the oxide layer was breached.

For the results included in this paper, we have tried a wider range of sample preparations and experimental procedures in an attempt to determine a trigger mechanism for the neutron emission. Most of the samples consisted of lathe chips of pure titanium metal, or titanium (67% aluminum, 6% vanadium, and 2% tin) alloy, or titanium (6% aluminum and 4% vanadium) alloy. Some electrolysis residue samples were used. The cleaning procedure normally included multiple washes with methylene chloride, methanol, and water. For about one-third of the samples, the fill temperature was raised to 400-500°C to activate the titanium for deuterium absorption.

For pure gas and clean sample conditions, the samples all absorbed deuterium gas after multiple LN temperature cycles.

Nineteen samples containing titanium metal and deuterium gas were prepared during the period between November 1990 and March 1991 for the measurements at Los Alamos.

RESULTS

Of the 19 samples prepared during the current set of experiments, only two gave excess neutron emission above the background levels. Many procedural variations were tried including deuterium gas loading at high temperature (400 to 500°C) gas loading at low temperature (-100° to 23°C), and temperature cycling from -197°C to 400°C inside the counting chamber. A typical sample was given 10 to 20 LN temperature cycles before stopping the measurements.

Sample DD-17

Our highest neutron emission was measured from sample DD-17. This sample contained 304 g of titanium (6,6,2) contained in a 1-ℓ stainless steel (SUS) sample bottle. The sample degassing was done at a maximum of 230°C using helium for the gas flushing to remove the air and cleaning agents.

During the neutron measurements, six LN temperature cycles were performed with a small amount (1 to 4 ℓ) of D_2 gas being absorbed during the warmup from LN temperature. A gas reserve tank attached to the sample cell was used to add the gas with the sample at LN temperature. On the seventh LN cycle, 17 ℓ of D_2 were accidentally added to the sample while at LN temperature. About 1 h into the warmup, a portion of the titanium chips went into a hot exothermic reaction excursion and all of the gas was absorbed in about 15 s. A localized spot on one side of the SUS bottle was hot; whereas, the rest of the bottle was still covered with frost. The bottle was immediately dunked into LN for 10 min and then removed from the LN and allowed to warm up in the detector.

During the first 2 h of the second LN warmup cycle, the sample emitted three bursts of neutrons as shown in Fig. 1. Detector system 4 has both inner and outer rings of ^3He as shown in Fig. 2. The 16 inner tubes have a counting efficiency of 31% and the 8 outer tubes have an efficiency of 5%. The ratio for the inner/outer detector efficiency is 6.22 as measured with a ^{252}Cf source (2.3 MeV). The detectors have independent electronics and the collection time bins for the inner detector were 200 s long and the outer time bins were 10 000 s. The ratio of the excess neutron counts in inner/outer rings was 6.2 ± 2.1 that compares well with the calibration ratio of 6.22 for ^{252}Cf neutrons.

After three additional LN temperature cycles with little or no excess neutron emission, sample DD-17 was moved to the ^3He detector (system 1) that was located in the underground tunnel (70 m deep) at Los Alamos to obtain a higher counting sensitivity. The first LN cycle in detector 1 consisted of a multiple cooling technique where the sample was recooled in LN a total of five times during the 6-h warmup period. The cold sample at -197°C was filled with 8 ℓ of D_2 gas and the temperature and pressure was monitored during warmup. The observable gas absorption process began when the temperature reached ~-100°C and the absorption rate increases with temperature. After about 1.5 ℓ of gas were absorbed and the temperature reached -30 to -10°C, the sample bottle was dunked into LN for ~1 min to cool the sample below -100°C and stop the absorption process. This process was repeated five times, after which the sample was left in the detector for ~5 d of counting.

Figure 3 shows the neutron coincidence counts collected in detector system 1 where the first large burst corresponds to the start of the first LN warmup counting cycle. Two bursts were observed during the multiple LN cycles and the excess activity continued for ~50 h with a total 15 bursts as shown in Fig. 3. In addition to the large bursts, there was an excess of small time-correlated events where only two neutrons were detected. The average control cell background rate in this detector is ~2 counts/d and during the excess activity period the doublet rate was three times higher than normal.

Sample DD-17 was temperature cycled eight more times over a 30-d period with negative results.

Sample Ti-48

Excess neutron emission was observed from sample Ti-48 that contained 56 g of Ti metal and sponge in a 250-mℓ stainless steel bottle. The titanium used in Ti-48 had had prior deuterium exposure through D_2O electrolysis experiments. The titanium consisted of 35 g of sponge, 11 g of metal pieces, and 10 g of 1.5-mm-thick titanium plate with a thin layer of palladium deposited on one side. The sample was evacuated at 220°C, and filled with 53 atm of deuterium gas. The gas pressure slowly decreased to 43 atm during the 90-d measurement period.

The measurements of sample Ti-48 were performed in detector 2 (see Table IV). Figure 4 shows the control runs for system 2 over a six-month period. Each data interval in Fig. 4 corresponds to the average of approximately 24 h of data collection. The control sample was a 300-mℓ stainless steel bottle containing 100 g of titanium chips in air. Prior experiments[6] had demonstrated that control runs with air or H_2 gas gave the same results.

Figure 5 shows the data from sample Ti-48 in detector 2 where each data interval corresponds to the average coincidence rate for ~24 h of data collection. The control runs are shown interspersed between the sample runs. There are several days with excess neutron emission from sample Ti-48 with the highest day having an average yield of 1.12 counts/h and a statistical significance of 6 σ. If we take the average of all the Ti-48 sample days and compare it to the control sample backgrounds, we obtain a 4-σ significance level.

SUMMARY

During the past two years, we have performed experiments to measure neutron emission from pressurized D_2 gas mixed with various forms of titanium metal chips and sponge. Our recent experiments have focused on determining a trigger mechanism for the anomalous low-level neutron emission. Thus far we have been unsuccessful in isolating a trigger mechanism, although we have measured several samples that yielded excess neutrons above background. To improve our detection sensitivity, we have increased the shielding in our counting laboratory and we have located additional detector systems in deep underground counting stations at Los Alamos and Leadville, Colorado.

We are using ^3He neutron detectors in a CH_2 moderator. The overall efficiencies range from 20% to 44% for the four separate detector systems that are operating in parallel experiments. Two of the detector systems are segmented to provide separate signal outputs for a consistency check on the origin of the signals. We measure both single (random) neutrons and time-correlated (coincidence) neutrons. Our coincidence background is dependent on the detector and shielding location and ranges from 2 counts/h to less than 0.5 counts/wk in the deep mine locations.

Only two of the 19 samples yielded excess neutron emission during the current series of experiments; however, the excess yields were observed in three independent detector systems (detectors 1, 2, and 4). The neutron yield from sample DD-17 in detector 1 was several orders or magnitude above the control run background levels, and the yield was the largest that we have observed during two years of experiments. This result was obtained in the low background underground laboratory at Los Alamos.

Our search for a trigger mechanism for the neutron emission has been unsuccessful and our sample success rate is less now than it was one year ago. We think that part of the reason for the low success rate is that we have tried a large variation in sample types and experimental

procedures. The number of experimental variables far exceeds our capacity to investigate the parameters.

ACKNOWLEDGMENTS

The authors thank S. Jones for his advice and help in the experiments. The assistance of J. Baca and O. M. Rivera was essential for the sample preparation.

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FIGURE CAPTIONS

Fig. 1. Neutron coincidence results for sample DD-17 in detector system 4 where the counting intervals are 200 s for the inside ring of tubes (lower graph) and 10 000 s for the outside ring of tubes (upper graph).

Fig. 2. Schematic diagram of detector system 4 showing the 3He tubes and the signal processing electronics including the amplifiers (A), shift registers (SR), total scaler (T), and coincidence scaler (R). The inside and outside rings of ^{3}He tubes have independent electronics and the inside/outside count ratio is used as a consistency check.

Fig. 3. Neutron coincidence results for sample DD-17 in detector system 1 where the counting intervals are 100 s. The top graph shows a time expansion of the active period. The 280 h of data prior to the active period correspond to the control sample or an inactive sample (DH-13) in the detector including LN temperature cycles.

Fig. 4. Neutron coincidence background (control sample) rate in detector system 2 where each data interval corresponds to the average rate for ~24 h of counting. Normally an LN cycle would begin the data interval.

Fig. 5. Neutron coincidence results for sample Ti-48 in detector system 2 where each data interval corresponds to the average rate for ~24 h of counting. Most of the time intervals were initiated with an LN temperature cycle.

TABLE I. Neutron Detectors Used for Cold Fusion Experiments

Type	Neutron Energy	Reaction	Typical efficiency (%)	γ Sensitive
^{3}He tubes	Thermal	^{3}He (n,p)	1-44	No
BF_3 tubes	Thermal	^{10}B (n,α)	0.5-20	No
H_2O + NaI/Ge	Thermal	H (n, γ)	<0.1	No
Activation foils	Thermal	(n, γ) or (n,F)	0.1-5	No
Li glass	Thermal	^{6}Li (n,α)	1-20	No
Liquid scintillator	Fast	n,p recoil	1-25	Yes
Plastic scintillator	Fast	n,p recoil	1-20	Yes
Plastic combination	Fast/Thermal	(n,p) + (n,α)	10-25	Yes/No
Cerenkov	Thermal	(n, γ) + e	15-20	High energy

TABLE II. Potential Sources of False Signals	
Electrical Noise	
1	Tube hv leakage (moisture seal)
2	EMI noise pickup (EMI seal)
3	Power line noise (filters, veto counters, etc.)
4	Microphonics
Cosmic-ray Background	
1	Total counts (cosmic-ray interactions in the shielding)
2	Coincidence counts (spallation in the detector)
3	D_2 target reactions
Area Background Neutrons	
1	Accelerators and reactors
2	Radioactive sources (manmade)
3	Natural radioactivity (uranium, radon)

Table III. Protection Against False Signals	
1	EMI shielded signal lines and hv
2	Hermetically sealed and dried hv components
3	Power-line noise filters
4	Multiple independent counter segments
5	Two (or more) different type detectors
6	Neutron spectral energy
7	Pulse time of arrival (slowing-down)
8	External veto detectors
9	Variable distance detectors
10	Pulse shape analysis
11	Cosmic-ray shielding (underground)
12	Rigorous control runs

TABLE IV. Neutron Detector Characteristics

Identification	Shape[a]	Size	No. ^{3}He Tubes	^{3}He Pressure (atm)	Cavity Size (cm)	Total Efficiency[b] (%)	Singles Bkg (s^{-1})	Coincidence Bkg (h^{-1})
System 1	Rectangular	25 x 35 x 35 cm^3 channel	18	4	12 x 23 x 35	21	0.19	0.1
System 2	Cylindrical	23 cm ϕ x 37 cm cavity	6	4	5(diam) x 20	26	0.07	0.5
System 3	Cylindrical	22 cm ϕ x 35 cm cavity	16	6	9(diam) x 28	34	0.16	1.6
System 4	Cylindrical	22 cm ϕ x 35 cm cavity (inside)	16	4	9(diam) x 28	31	0.39	1.8
		(outside)	8	4	same	5	0.22	0.1

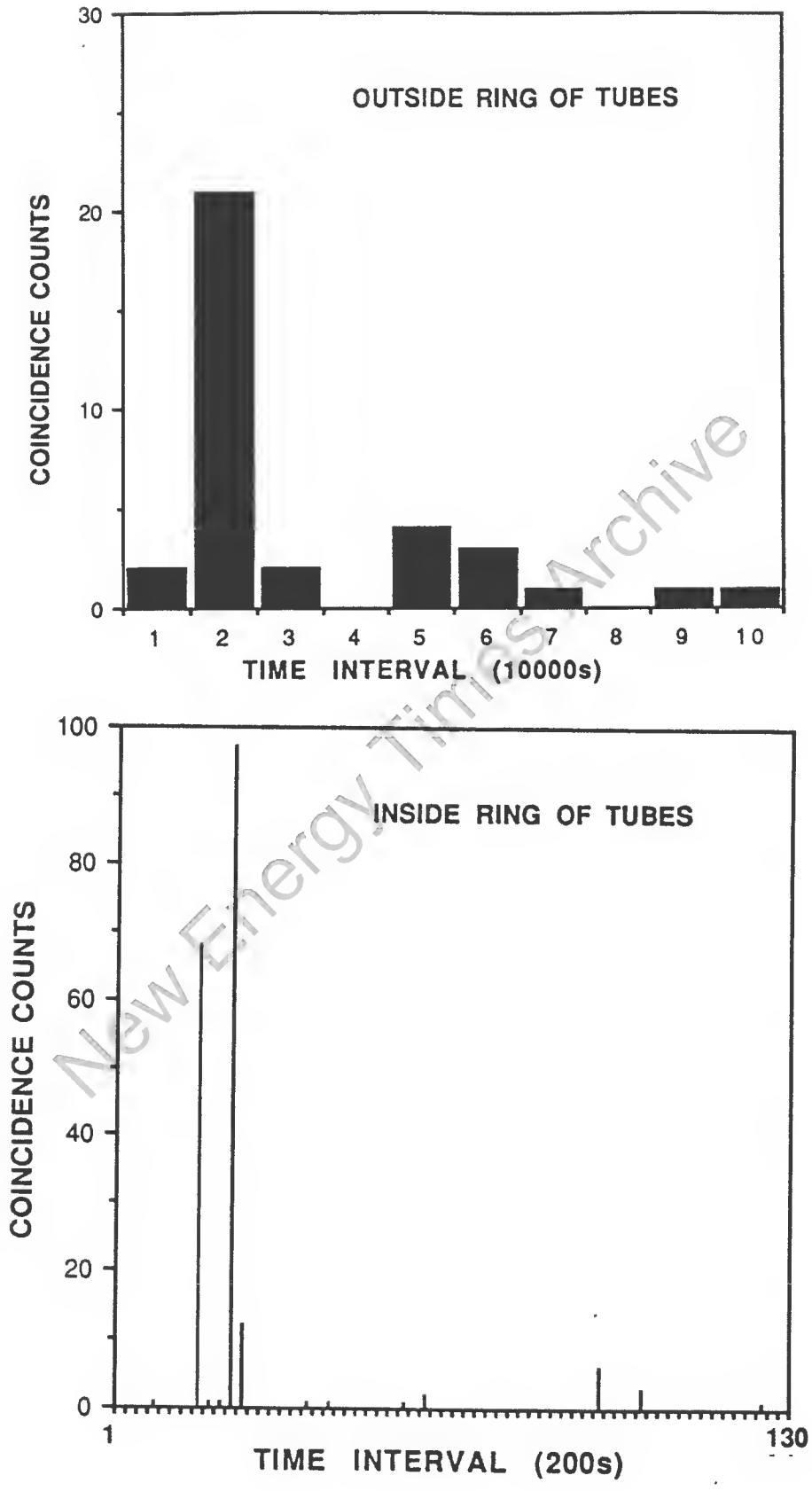


Fig. 1

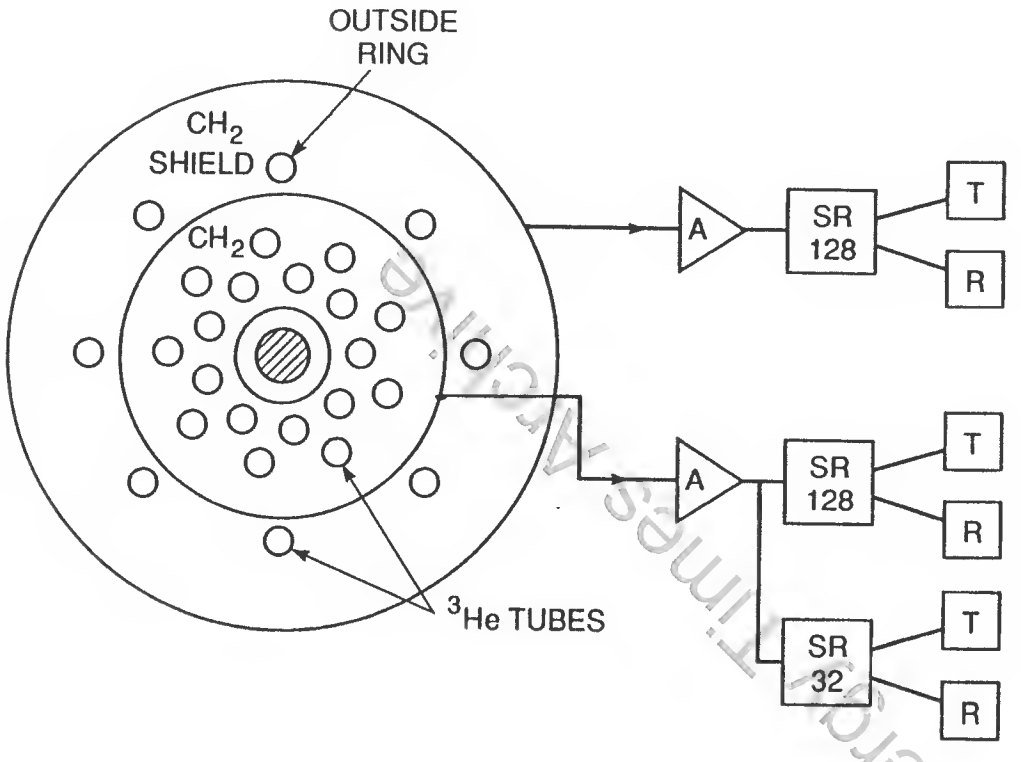


Fig 2

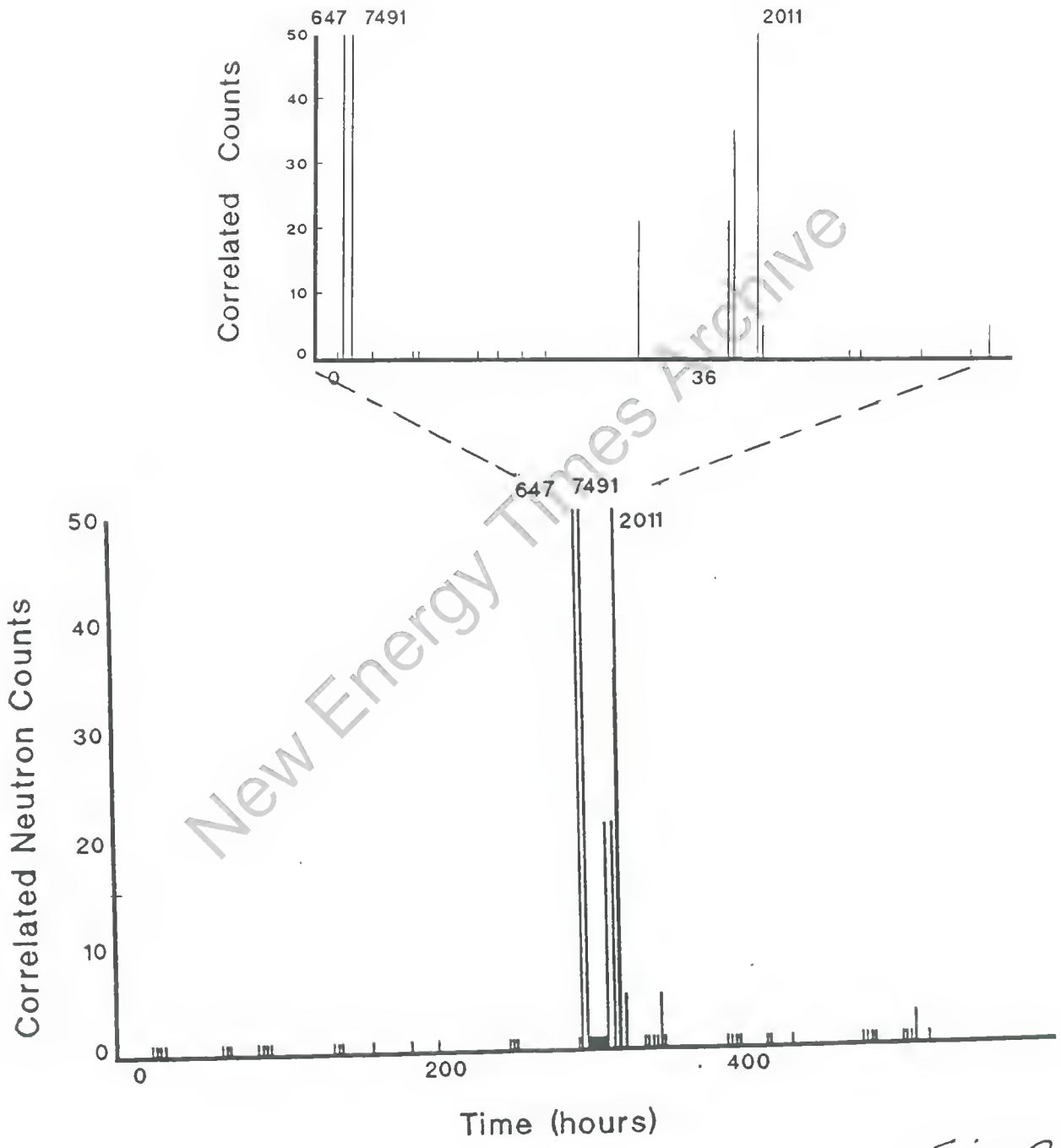
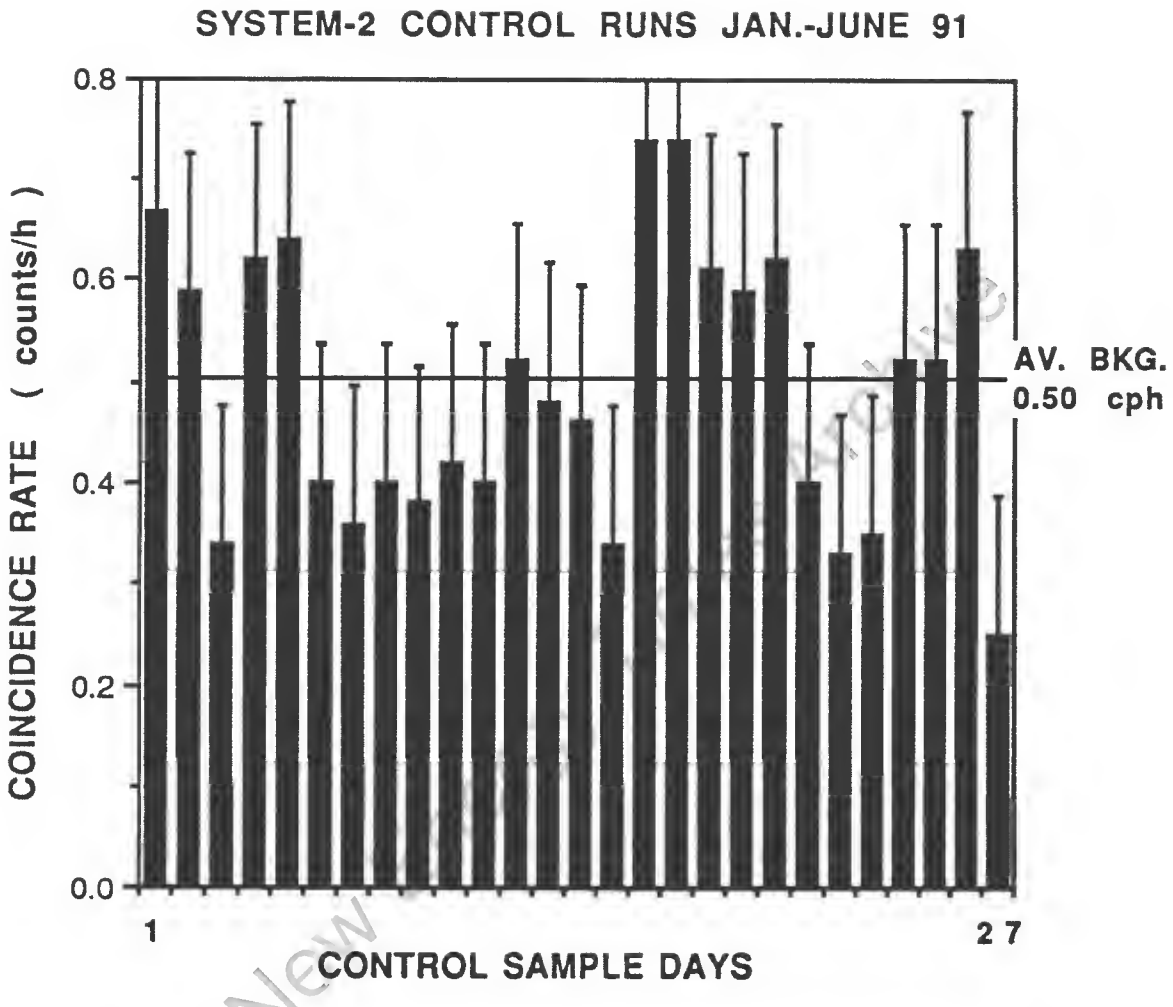


Fig. 3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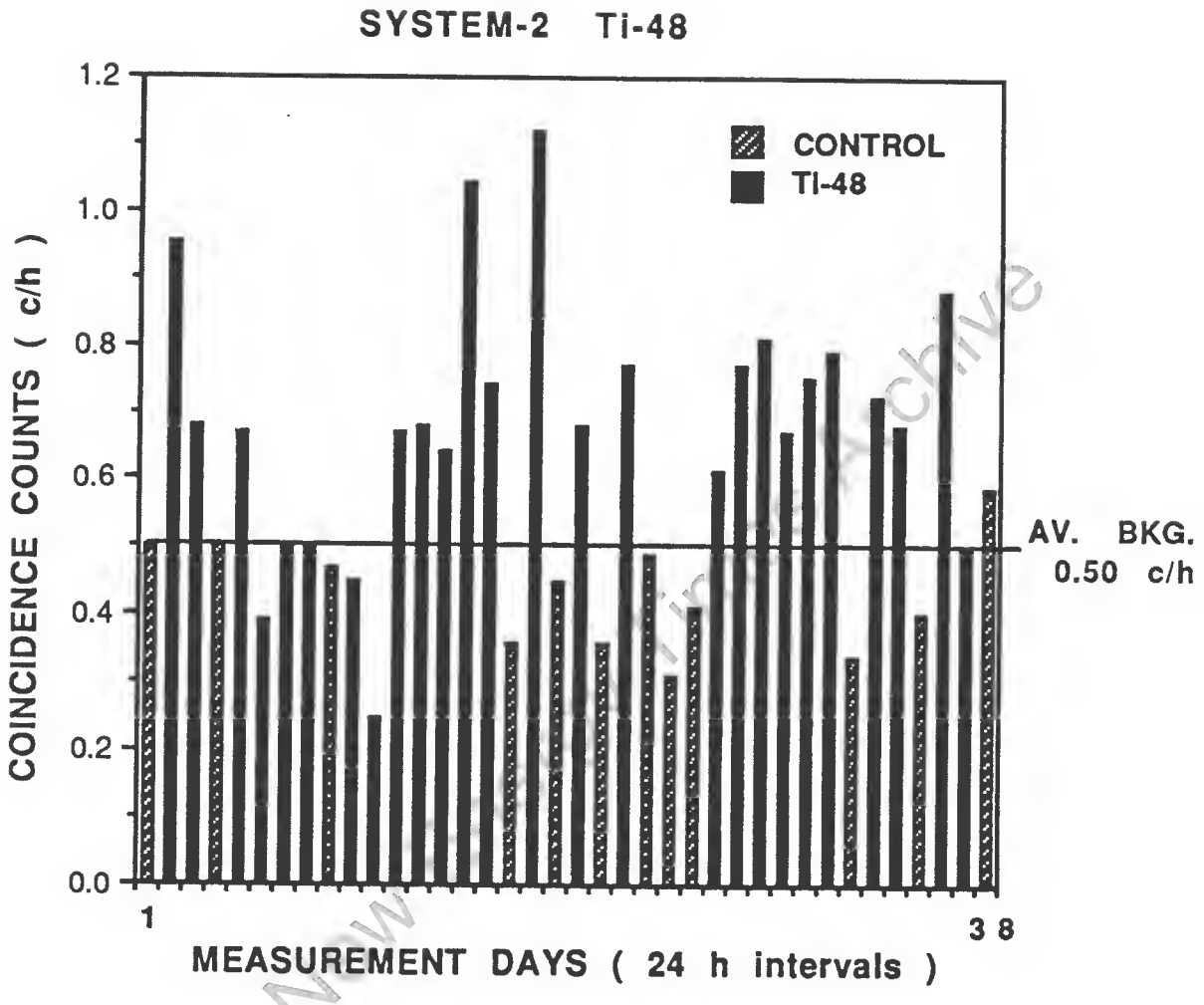
1
27
CONTROL SAMPLE DAYS

Fig 4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CONTROL
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COINCIDENCE COUNTS (c/h)
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0.8
0.6
0.4
0.2
0.0
1
38
MEASUREMENT DAYS (24 h intervals)

Fig 5